MONTHLY NOTICES

OF THE

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Professor H. H. TURNER, D.Sc., F.R.S., PRESIDENT, in the Chair.

Henri Deslandres, D.-ès-Sc., F.R.A.S., Observatoire d'Astronomie Physique, Meudon, S.-et-O., France;
Charles D. Perrine, Lick Observatory, Mount Hamilton, California, U.S.A.; and
George W. Ritchey, Yerkes Observatory, Williams Bay, Wisconsin, U.S.A.,

were balloted for and duly elected Associates of the Society.

Edward Barlow, Harewood, Andover, Hants;
Otto J. Klotz, LL.D., Department of the Interior; and 437 Albert Street, Ottawa, Canada; and
Major C. Leigh-Lye, care of Messrs. Holt & Co., 3 Whitehall Place, S.W.

were balloted for and duly elected Fellows of the Society.

The following Candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended :—

William Allan, M.A., B.Sc., 88 Leamington Terrace, Edinburgh (proposed by P. S. Hardie);
Ernest Cuthbert Atkinson, M.A., formerly Assistant Curator of the Temple Observatory, Rugby, Erwood, Beckenham, Kent (proposed by H. H. Turner);
Arthur Henry Bagnold, Colonel R.E., Superintendent of the Building Works Department, Royal Arsenal, Warren Road, Shooter's Hill, S.E. (proposed by Major P. A. MacMahon);

The Rev. D. B. Marsh, D.Sc., Hamilton, Ontario, Canada (proposed by John A. Brashear); and
William Newbold, Tonbridge School, Tonbridge, and 7 Broadwater Down, Tunbridge Wells (proposed by Edward Weldon).

Fifty presents were announced as having been received since the last meeting, including amongst others:—

Twenty-one prints of the Astrographic Chart (Zones +65°, +66°, +67°), presented by the Royal Observatory, Greenwich; two plates of the Harvard photographic map of the sky (to complete series), presented by the Harvard Observatory.

Analyses of Errors of Moon's Longitude for Inequalities of Longer Periods. Methods and Results. By P. H. Cowell.

Up to the present I have confined myself to short-period terms, and I have taken 400 lunar days as the period of analysis. To this there have been two exceptions. In order to properly discuss the terms $\sin g$ and $\sin(2D-g)$ it was necessary to analyse for $\sin(D-g)$; and similarly for terms $g' \pm D$ it was necessary to analyse for $\sin g'$.

Although the analysis for short-period terms will not be finally left in its present state, I have now gone on to terms of longer period. The whole fifty-four years, 1847–1901, has now been taken as a single period of analysis.

The errors to be analysed are shown in Table I.; the Hansen period 1847–1901 has been divided into 48×400 lunar days, or 480 times 40 lunar days, numbered from 891 to 1330 inclusive, in order to permit the Airy period beginning with No. 1. Each period of forty lunar days I refer to as a column, as the original errors have been arranged in columns, each column containing the errors of such observations as occurred in the corresponding forty lunar days. Table I. exhibits the mean error for each column, each individual observation having been corrected for thirteen corrections, as described in previous papers; but the constants for each period of 400 lunar days, which was removed for the purpose of the short-period analyses, have now been restored, and a correction $+1''\cdot3$ applied throughout the whole period, reducing the sum of the 480 quantities here tabulated to $+1''\cdot1$. It is further necessary to state that there were no observations in column 1244—that is to say on line 34, strip 10, of Table I.—and the number there entered has been supplied by inspection.

Mean Error of Moon's Longitude for each of 480 Columns of Forty Lunar Days each in Tenths of a Second of Arc.

	Columns											
	851 to 890.	891 to 930.	931 to 970.	971 to 1010.	1011 to 1050.	1051 to 1090.	1091 to 1130.	1131 to 1170.	1171 to 1210.	1211 to 1250.	1251 to 1290.	1291 to 1330
	1	2	3	4	5	6	7	8	9	10	11	12
1	+27	− 3	−18	−34	−21	0	+25	− 2	+13	+12	+28	− 1
2	+29	− 8	−30	−56	−19	+ 6	+32	− 8	+15	+17	+27	− 4
3	+29	−14	−21	−60	−11	−10	+30	−34	+12	+ 9	+33	− 6
4	+16	−22	− 3	−61	−18	+ 7	+32	− 5	+20	+19	+22	− 8
5	+16	− 9	−21	−53	−19	− 5	+29	+17	+29	+20	+19	− 8
6	+17	− 6	−12	−61	−23	−17	+16	0	+22	+48	+26	0
7	+21	−23	−17	−65	− 8	− 4	+21	+ 4	+11	+17	+ 2	+ 4
8	+27	−27	−17	−35	−18	+ 5	+21	+11	+12	+32	+17	− 4
9	0	+ 5	−27	−33	−19	+10	+30	+21	+25	+40	+21	− 9
10	+ 4	+ 9	−31	−39	−14	+ 2	+19	− 2	+19	+21	+35	−13
11	+14	+ 1	−28	−37	+ 5	0	+26	+ 3	+28	+30	+16	−14
12	+15	−21	−15	−52	− 4	+24	+20	+ 3	+14	+22	+26	−10
13	+ 1	−10	−21	−52	−10	+19	+29	− 5	+20	+11	+22	−11
14	+ 7	−18	−19	−58	−11	+16	+26	−10	+24	+ 4	+18	−14
15	+16	−17	−33	−53	0	+12	+26	+ 6	+ 7	+14	+ 9	+ 3
16	+24	−23	−31	−61	− 8	+12	+21	+ 6	−14	+ 6	+ 7	+ 5
17	+ 6	−15	−48	−35	0	+16	+24	+19	+ 2	+20	+13	+ 2
18	− 2	−11	−50	−32	−15	+10	+27	+12	+17	+20	+ 9	− 6
19	− 6	−12	−32	−31	− 1	+20	+23	+12	+21	+25	0	− 1
20	+17	− 6	−27	−44	− 9	+10	+25	+ 7	+31	+16	+14	− 2
21	+ 3	− 8	−27	−46	+ 1	+14	+24	+ 6	+35	+19	− 3	−14
22	+ 9	−14	−22	−35	+ 5	+12	+18	− 3	+29	+19	+26	+ 7
23	+13	−18	−27	−48	+10	+ 7	+22	− 1	+32	+ 9	+14	−23
24	+ 2	−20	−24	−34	+ 8	+ 2	+13	− 6	+ 3	+ 5	+ 3	+ 5
25	0	−25	−42	−38	+ 2	+13	+12	+ 1	+ 7	+25	+ 1	− 5
26	0	−16	−46	−28	− 4	+21	+13	+10	+ 3	+15	+10	+ 6
27	−14	−10	−46	−45	− 6	+14	+14	− 1	+17	+22	+10	+ 6
28	−16	−30	−42	−29	+10	+22	+ 5	+ 3	+28	+29	+14	− 5
29	+ 6	−13	−24	−32	+ 7	+25	+15	+ 6	+26	+31	+13	+ 3
30	+ 8	− 7	−40	−35	− 4	+15	+24	+19	+21	+23	+ 1	− 4
31	+ 7	−14	−27	−27	+13	+10	+10	+10	+26	+20	+10	− 4
32	+17	−12	−33	−19	− 8	+17	+ 9	+ 1	+25	+25	− 5	+ 6
33	0	−21	−35	−35	− 3	+28	0	+14	+ 3	+49	− 8	− 5
34	− 7	− 9	−36	−25	−14	+21	− 5	+ 8	+22	[+33]	+ 2	−16
35	− 6	−12	−55	−26	−14	+13	+ 2	+25	+21	+37	− 4	−25
36	−12	0	−46	−29	−13	+12	+ 2	+15	+17	+35	+ 4	− 6
37	−21	−12	−46	−23	−10	+16	+ 3	+16	+23	+30	+ 9	−12
38	−20	−12	−55	−14	+ 6	+17	− 2	+16	+28	+29	+21	− 1
39	− 2	−24	−37	− 9	− 1	+ 8	+11	+18	+33	+23	+ 8	− 5
40	− 5	−15	−28	−32	+ 7	+22	− 3	+13	+25	+27	− 5	+12

Taking $\pm 1''\cdot 7$ as the probable error of a single observation, the quantities in Table I. will be subject to an accidental error of $\pm 0''\cdot 5$. This is approximately the accidental error of a star.

It will be noticed that I refer to the columns of Table I. as strips. This is partly because I use the word "column" as just explained to denote forty lunar days and partly with reference to my method of analysis, which I now explain in detail for the argument E−J, a Jupiter term. The columns of Table I. are used by me in MS. written on separate strips of paper, 2 inches wide, with the numbers 1–40 printed down them. A strip blank, except that it is marked with the consecutive numerals, is then placed on the table, and column 1091, that is to say, the first entry of the seventh strip, is placed in a line with the "1" on the blank strip; column 1092 will then fall opposite the "2," and so on. Column 851, the first of the first strip, is placed opposite the "2" of the blank strip; and the first column of the ninth strip opposite the "4" of the blank strip, the complete arrangement being indicated by the annexed table :—

Column.	Number of Blank Strip.	Column.	Number of Blank Strip.
851	2	1091	1
891	13	1131	12
931	24	1171	4
971	16	1211	15
1011	27	1251	7
1051	19	1291	18

The arrangement is so contrived that entries opposite the same number of the blank strip correspond as nearly as possible to the same value of the argument E−J, and may properly be added for an E−J analysis. Moreover, the value of the argument E−J for any line is found by multiplying the number on the blank strip by the "unit," an exact fraction if possible of 360° or a low multiple of it. It is clearly necessary that the "unit" shall be a close approximation to the movement of E−J in 40 lunar days. In the present instance $\frac{3\times 360^\circ}{29}$ has been taken as the unit, so that the values of E−J recur every twenty-ninth line of the blank strip. I postpone an account of the calculations justifying the precepts of the foregoing table until the rest of the analysis has been described. The numbers on the strips are then added horizontally and the results shown in the following table :—

No. on Blank Strip.	Sum.	No. of Quantities added.	No. on Blank Strip.	Sum.	No. of Quantities added.	No. on Blank Strip.	Sum.	No. of Quantities added.
1	+ 25	1	30	− 18	12	59	−49	2
2	+ 59	2	31	− 30	12	60	−60	2
3	+ 59	2	32	+ 6	12	61	−69	2
4	+ 74	3	33	+ 8	12	62	−50	2
5	+ 60	3	34	− 27	12	63	−38	2
6	+ 44	3	35	− 40	12	64	+ 6	1
7	+ 86	4	36	− 72	12	65	− 1	1
8	+ 98	4	37	− 14	12	66	+ 7	1
9	+112	4	38	+108	12	...	...	...
10	+ 52	4	39	− 36	12	...	...	...
11	+ 61	4	40	− 97	12	...	...	...
12	+ 83	5	41	− 19	11	...	...	...
13	+ 54	6	42	− 19	10	...	...	...
14	+ 30	6	43	+ 15	10	...	...	...

No. on Blank Strip.	Sum.	No. of Quantities added.	No. on Blank Strip.	Sum.	No. of Quantities added.
29	−64	12	58	−41	3
28	−80	12	57	− 3	4
27	−65	12	56	−27	4
26	+ 7	11	55	−43	5
25	−24	11	54	+ 1	6
24	+ 1	11	53	−30	6
23	+79	10	52	−41	7
22	+27	10	51	−38	8
21	+ 8	10	50	−11	8
20	+28	10	49	− 4	8
19	−28	10	48	−12	8
18	+22	9	47	+31	8
17	+32	8	46	− 2	9
16	+70	8	45	−34	9
15	+61	7	44	− 1	9

Owing to the recurrence every twenty-ninth line, the entries for Nos. 1, 30, 59 may properly be added. Moreover, since the sum of the values of E−J for the pth and $29-p$th lines is 360°, the values of cos (E−J) are the same throughout the whole of a horizontal line of the preceding table; while the values of sin (E−J) are the same numerically, but the last two are of changed sign.

Hence we obtain the following table, sufficiently explained by its headings :—

Multiples of Unit of $\frac{360°\times 3}{29}$	Value of E−J.	Sum for Sine Analysis.	100 × sin E−J.	Product.	Sum for Cosine Analysis	100 × cos (E−J).	Product.
0	0·0	...	0	...	−105	+100	−105
1	37·2	+41	+ 60	+25	−125	+ 80	−100
2	74·5	+61	+ 96	+59	−123	+ 27	− 33
3	111·7	+32	+ 93	+30	− 40	− 37	+ 15
4	149·0	+55	+ 51	+28	+ 9	− 86	− 8
5	186·2	+24	− 11	− 3	− 34	− 99	+ 34
6	223·4	−28	− 69	+19	+ 48	− 73	− 35
7	260·7	+24	− 99	−24	+ 2	− 16	0
8	297·9	+94	− 88	−83	+ 88	+ 47	+ 41
9	335·2	−20	− 42	+ 8	+ 28	+ 91	+ 25
10	12·4	+56	+ 21	+12	− 24	+ 98	− 23
11	49·6	−89	+ 76	−67	+ 17	+ 65	+ 11
12	86·9	+34	+100	+34	+ 94	+ 5	+ 5
13	124·1	− 1	+ 83	− 1	+ 71	− 56	− 40
14	161·4	−15	+ 32	− 5	+105	− 95	−100
Sums	...	...	...	+32	+ 11	...	−313

In the last line the sum +11 should obviously be the total of the entries of Table I., which, as previously noted, is +11 ; this checks nearly all the arithmetic. The other two sums must be divided by

$$\Sigma \sin^2 (E-J) \text{ and } \Sigma \cos^2 (E-J)$$

respectively : each of these quantities is 240. Dividing, however, by 2400 we get the result expressed in seconds of arc.

It is as follows :—

Hansen's tabular places exhibit an excess over the observations of

$$+0''\cdot01 \sin (E-J) - 0''\cdot13 \cos (E-J).$$

Both these terms may be discarded as accidental.

Before proceeding to give the results of similar analyses, I must explain the calculation of the table of precepts for the arrangement of the twelve strips for the additions.

From the table published at the end of my paper in the *Monthly Notices* for March I obtain E−J = 21°·4980 at the middle of my forty-fourth period of analysis, and its movement in forty lunar days = 37°·36605.

The latter quantity suggested the unit

$$\frac{3 \times 360^\circ}{29} = 37^\circ{\cdot}24$$

and in terms of this unit E−J = 1·73178÷3 at the middle of the forty-fourth period of analysis and the movement in forty lunar days is 3·010044÷3. It is convenient to keep the "3" in evidence in the manner indicated.

The interval from the middle of the forty-fourth period of analysis to the middle of the first strip is 435 columns of forty lunar days, and the movement in this time is

$$1309{\cdot}36914 \div 3,$$

or discarding multiples of 29

$$4{\cdot}36914 \div 3.$$

Hence the value for the middle of strip 1 (that is to say, strip 1, line $20\frac{1}{2}$) is

$$6{\cdot}10092 \div 3.$$

It is convenient to add 58 to this, making 64·10092, so that on division by 3 the fraction lies between $\frac{1}{3}$ and $\frac{2}{3}$. Thus, for strip 1, line $20\frac{1}{2}$ E−J = 21·37 units. Therefore, strip 1, line 1, or column 851, E−J = 2 units.

Continuing, in one strip or forty columns, the movement of E−J = 120·40÷3.

Adding ·40 to ·10 (from 64·10092 above), the unit is unchanged, the division by 3 leaves the fraction between $\frac{1}{3}$ and $\frac{2}{3}$; there is no break in continuity, and column 891 corresponds to unit 2+40−29 = 13.

Adding another ·40, making ·90, there is still no break of continuity, and column 931 corresponds to 13+40−29 = 24.

Adding another ·40, making 1·30, we now have to add 29, making 0·30 on casting out threes.

A discontinuity of 10 is therefore introduced, and column 971 corresponds to

$$24+40-58+10 = 16$$

and so on.

It is clear that since 360° = $9\frac{2}{3}$ units, the discontinuity of 10 units is really a discontinuity of $\frac{1}{3}$ unit, and at the middle of each strip the true and adopted value of E−J never differ by more than $\frac{1}{6}$ unit or 6°·2.

Almost every figure used in the E—J analysis has now been set down, and it will be seen that the analysis for fifty-four years is reduced to a quite short computation when once the method has been reduced to routine. I have carried through the necessary computations for four different arguments in one day.

The following table gives the outline of the various analyses performed :—

Argument	g'	$g'-\omega+\omega'$	$(2g'-\omega+\omega')$	$(2g'+2\omega')$	$(\nabla-E)$	$(2\nabla-3E+85°)$	$(E-J)$
Unit	$\frac{360°}{9}$	$\frac{360°}{10}$	$\frac{3\times360°}{14}$	$\frac{5\times360°}{21}$	$\frac{360°}{14}$	$\frac{360°}{35}$	$\frac{3\times360°}{29}$
Movement in 40 lunar days							
in degrees	40°·80584	36°·19540	77°·00524	86°·00040	25°·52522	10°·24425	37°·36605
in units	1·020146	1·005428	2·994648÷3	5·016690÷5	0·992647	0·995969	3·010044÷
Value at middle of 44th period of analysis							
in degrees	4°·1375	57°·8240	61°·9615	141°·1374	47°·5763	76°·5091	21°·4980
in units	0·10344	1·60622	2·40961÷3	8·23301÷5	1·85019	7·43848	1·73178÷
Value in units corresponding to columns							
851	1	9	5	7	8	1	2
891	6	10	12	1	6	6	13
931	2	10	10	20	4	11	24
971	7	10	8	14	1	16	16
1011	3	10	6	8	13	21	27
1051	7	1	4	6	11	25	19
1091	3	1	11	21	8	30	1
1131	8	1	9	15	6	35	12
1171	4	1	7	13	4	5	4
1211	9	1	5	7	2	10	15
1251	4	2	12	1	13	15	7
1291	9	2	10	20	11	19	18
tab—obs. apparent coefficients of							
sin (arg.)	−0″·35	−0″·09	+0″·02	+0″·25	−0″·34	+0″·29	+0″·01
cos (arg.)	−0·28	+0·32	−0·08	−0·02	−0·02	0·00	−0·13
sin (twice arg.)	+0·05	−0·17	+0·20	−0·03	+0·10		
cos (twice arg.)	−0·04	+0·01	−0·04	−0·06	−0·01		

The arguments $\omega-\omega'$, ☊, $2M-E+49°$ move so slowly that 5×40 lunar days were taken as the unit of time, and the following table was formed from Table I. by throwing two columns into one, and adding five lines at a time. Each entry is therefore the mean for 200 lunar days expressed in units of 0″·02.

	A.	B	C.	D.	E.	F.
1	+117	− 93	−88	+148	+ 89	+129
2	+ 69	−104	−82	+107	+ 89	+101
3	+ 53	−116	−20	+127	+ 93	+ 91
4	+ 39	−188	−33	+120	+ 57	+ 43
5	+ 27	−142	+26	+ 89	+106	+ 41
6	− 16	−198	+ 3	+ 71	+ 95	+ 48
7	+ 11	−186	−26	+ 16	+ 97	− 5
8	− 60	−212	−11	+ 11	+126	+ 37
9	− 56	−264	− 2	− 32	+ 77	− 27
10	− 42	−233	− 4	+ 34	+158	− 22
11	− 65	−252	+71	− 3	+ 81	− 46
12	− 67	−203	+68	+ 56	+ 87	− 2
13	− 85	−201	+48	− 3	+ 77	− 30
14	− 76	−169	+97	+ 37	+120	+ 6
15	− 68	−132	+89	+ 58	+164	− 44
16	− 63	−107	+75	+ 78	+144	− 12

The same plan of analysis was adopted, and the six strips are referred to by the letters A to F. The outline is given in the tables below :—

Argument.	$2M-E+49^\circ$.	$\omega-\omega'$.	$-\Omega$.	2ω.
Unit	$\frac{360^\circ}{28}$	$\frac{360^\circ}{16}$	$\frac{360^\circ}{33}$	$\frac{3\times360^\circ}{16}$
Movement in 200 lunar days in degrees	12°·92915	23°·05215	10°·96205	68°·0478
in units	1·005601	1·024540	1·004855	3·02435÷3
Value at middle of 44th period of analysis in degrees	54°·4667	306°·3135	326°·9251	25°·4894
in units	4·2363	13·6139	29·9681	1·1329÷3
Value in units corresponding to A 1	4	3	15	2
B 1	20	4	31	13
C 1	8	4	14	13
D 1	25	5	30	13
E 1	13	5	13	8
F 1	1	5	29	8
Tab.−obs. apparent coefficients of sin (arg.)	−0″·33	+0″·60	−1″·18	+0″·10
cos (arg.)	−0·06	−0·31	−0·28	−0·15
sin (twice arg.)	−0·01	0·00	...	...
cos (twice arg.)	−0·05	−0·03	...	...

It should be noticed that $-\mathfrak{g}$ and not $\mathfrak{g}$ has been taken as the argument.

All the long-period analyses at present completed have now been exhibited. It is clear that many of the coefficients obtained may be treated as accidental. Further it is to be remembered that all coefficients given are "apparent" (see *Monthly Notices*, March, p. 413). I proceed now to the explanations. The quantities are always spoken of in the sense *tabular* minus *observed*

(i.) $$-0''\cdot35 \sin g'$$

On p. 419 I found from a former analysis

$$+0''\cdot24 \sin (g'-\mathrm{D})-0''\cdot83 \sin g'+0''\cdot81 \sin (g'+\mathrm{D})$$

I have subsequently applied the correction $+0''\cdot55 \sin g'$, whose apparent effect, according to the ideas of p. 413, will be

$$+0''\cdot55 \sin g'-0''\cdot27 \sin (g'-\mathrm{D})-0''\cdot27 \sin (g'+\mathrm{D})$$

We ought now to have

$-0''\cdot28 \sin g'+0''\cdot54 \sin (g'+\mathrm{D})$. The former term is here verified to within $0''\cdot07$. It is clearly the apparent effect of the latter term which alone is real.

(ii.) $-0''\cdot28 \cos g'$. On p. 419 $-0''\cdot33 \cos g'+0''\cdot46 \cos (g+\mathrm{D})$ was found. Again the latter term is alone real.

We have therefore real terms

$$+0''\cdot54 \sin (g+\omega-\omega')+0''\cdot46 \cos (g+\omega-\omega')$$

As I said on p. 420, the true argument may be $g+\varpi$, or the *Venus* term $g+2\varpi+3\mathrm{V}-5\mathrm{E}$, and it is necessary to wait till the Airy period is reduced. My present supposition is—but it is no more than a supposition—that the term is partly $+0''\cdot4 \cos (g+\varpi)$, with possibly a correction to the coefficients of $\sin (g+\omega-\omega')$ and the *Venus* term. A term $+0''\cdot4 \cos (g+\varpi)$ might arise in this way. Suppose the observed declinations on the average $1''$ too small. Remove the constant correction $-1''$ that Hansen applies to all latitudes. If, then, $+1''$ be applied to all tabular latitudes, and $+1''$ to all observed declinations, the difference "tabular *minus* observed" longitude receives a correction $-0''\cdot4 \cos (g+\varpi)$, which would cancel part of the terms that result from my analysis of the errors. Errors of refraction or division error may account for part of the supposed error of $1''$ in the observed declinations, but not, I think, for all of it. The outstanding part may possibly be due to an erroneous parallax of the Moon, which is, as far as I can judge, a not very accurately determined quantity.

(iii.) $-0''\cdot09 \sin (g-D)$ must be taken with $+0''\cdot17 \sin g$ (see p. 419, l. 13). The latter is the real term, the former its apparent effect.

(iv.) $+0''\cdot32 \cos (g-D)$. On p. 417 I gave $+0''\cdot34 \cos (g-D)$. It there appears as if this is the real term, and cos $(g-2D)$, cos g merely the apparent terms. Of course cos $(g-D)$ cannot be exactly the real term, but there may possibly be some term with an argument of nearly the same speed.

(v.) $-0''\cdot17 \sin (2g'-2\omega+2\omega')$. This is for the most part the apparent effect on an error of $0''\cdot22$ in the coefficient of the allied term $(-g-g'+\omega-\omega')$.

(vi.) $+0''\cdot20 \sin (4g-2\omega+2\omega')$. This term is nearly what we might expect. Hansen's tabular places contain the term $-0''\cdot134 \sin (4g'-2\omega+2\omega')$ (see *Monthly Notices*, January, p. 164), and the true coefficient is $-0''\cdot28$ (see Newcomb's Transformation, *Astron. Papers, Amer. Eph.* vol. i.).

(vii.) $+0''\cdot25 \sin (2g'+2\omega')$. This term is confirmed in the same way as (vi.). The tabular coefficient is $-54''\cdot98$, and the true $-55''\cdot2$.

(viii.) $-0''\cdot34 \sin (V-E)$.

Hansen's term is	$-1''\cdot10 \sin (V-E)$
Radau gives	$-0''\cdot86 \sin (V-E)$

Radau is therefore more nearly right, and is within the possible errors of observation.

(ix.) $+0''\cdot10 \sin (2V-2E)$. Hansen gives $+0''\cdot43$, Radau $+0''\cdot28$. Again Radau's term seems the better.

(x.) $+0''\cdot29 \sin (2V-3E+85°)$. Hansen gives no term. Radau gives $-0''\cdot35$, and is thus confirmed.

(xi.) $-0''\cdot33 \sin (2M-E+49°)$. This inequality is entirely masked by the term in ☊, which separates from it at the rate of one revolution a century. It follows, therefore, that fifty-four years is inadequate for a thorough discussion. In fact, if the terms subsequently found $-1''\cdot18 \sin (-☊) -0''\cdot28 \cos (-☊)$ be taken out and the analysis repeated, the present term would approximately change sign. About 1875 the two arguments $2M-E+49°$ and $-☊$ are equal; for a short time $-1''\cdot18 \sin (-☊)$ is indistinguishable from $-1''\cdot18 \sin (2M-E+49°)$, and although this effect is reduced by extending the analysis over fifty years, it still comes in with about two-thirds of its full effect, changing the sign as I have said.

Of course, if another century of observations were not so soon to be available it might be worth while to try and separate the two terms. Each coefficient could, however, only be obtained with a probable error several times as large as the probable error in ordinary cases. The process would bear some analogy to the separation of the parallactic inequality from the semi-diameter, or the cos D term from the mean error.

(xii.) $+0''.60 \sin(\omega - \omega')$.

Hansen's tables give	$+1''.58$
Hansen's theory gives	$+1''.33$
Delaunay gives	$+0''.87$

The evidence of the observations is therefore in favour of Delaunay.

(xiii.) I am unable to explain $-0''.31 \cos(\omega - \omega')$.

(xiv.) $-1''.18 \sin(-\mathfrak{Q}) - 0''.28 \cos(-\mathfrak{Q})$. The coefficients are uncertain to about $0''.3$ on account of the *Mars* term just discussed. It is clear, however, that Hansen's figure of Earth terms require considerable diminution. On the other hand, the term in $\sin \mathfrak{Q} \cos g$ apparently requires a large increase. Hansen's tables are equivalent to $+0''.59 \sin \mathfrak{Q} \cos g$; I have already applied $+0''.445 \sin \mathfrak{Q} \cos g$, bringing the coefficient up to $+1''.04$ in accordance with Hill's calculations. On analysing the coefficients of $\cos g$ and $\sin g$, given on pp. 415, 416 (last two columns), I obtain

$$(-0''.22 \sin \mathfrak{Q} + 0''.29 \cos \mathfrak{Q}) \cos g$$
$$+ (+0''.13 \sin \mathfrak{Q} + 0''.05 \cos \mathfrak{Q}) \sin g$$

The coefficient of $\sin \mathfrak{Q} \cos g$ should, therefore, apparently be $+1''.26$, or about double Hansen's value.

I ought to add that any at present undiscovered term with a period between fourteen years and twenty-five years (a very wide range) will affect an analysis for $\mathfrak{Q}$ based upon fifty-four years. This range will shortly be considerably reduced by taking the Airy period into the discussion. As no two discussions have hitherto given the same coefficient of $\cos \mathfrak{Q}$, I suspect the existence of such an undiscovered term.

The Parallactic Inequality: A Reply. By P. H. Cowell, M.A.

On p. 567 of the present volume of the *Monthly Notices* Professor Turner writes: "If the solar parallax as determined from observations of the Moon is affected with an entirely unknown systematic error, then it is a pity to publish the result." I thought I had made it clear on p. 96, in a passage that Professor Turner has quoted on p. 406, that I saw no means of separating the errors of observation from the error of tabular parallactic inequality, and that my results were given on the assumption that the errors of observation were zero. I see no objection to publishing a result if its limitations are clearly set down.

I said last December, and I still say now, that I am unable to determine how much of the coefficient of the parallactic in-

equality is to be attributed to errors of observation. I did say, however, that I thought probably not more than $\pm 0''.3$. If this estimate is correct the lunar observations give the solar parallax almost free from accidental error (that is to say, more observations under similar conditions are not required), but subject to an unknown systematic error possibly amounting to $0''.02$. I still adhere to the $0''.3$ given above, partly because I have since found a term $0''.28 \cos D$, which must be attributed to errors of observation, and partly because of the investigations of which I give some account at the end of the present paper, and which were undertaken in consequence of the suggestions of Professor Newcomb on p. 570.

The fundamental idea of Professor Turner was that the observations with another instrument should be examined with a view to seeing whether they gave the same parallactic inequality as the transit circle. It will be noticed that whatever the answer to this question may be it does not tell us much. For if the two instruments agree, they may both be wrong ; and if they disagree, there is nothing to show which is wrong.

Again, Professor Newcomb has suggested the exclusion of the daylight observations. I have carried out the necessary calculations with this modification of Professor Newcomb's plan, that because the six o'clock observations were rejected in summer, it was absolutely necessary to reject them in winter as well, for reasons which I set out later. It is a matter of judgment only, and not of absolute certainty, whether the six o'clock observations are so far inferior to the rest that it is better to leave them out altogether.

Following out his own idea, Professor Turner set out to examine fifteen years of the Old Altazimuth. He was looking for a discordance of possibly $0''.3$—I do not think he can have expected to find anything much larger than this. Under these circumstances he was, in my opinion, bound to keep his accidental error under a probable value of $0''.2$, or even less ; but let us say $0''.2$. On p. 96 I stated that the weight of the parallactic inequality found from 5647 observations of unit weight was 515. It would therefore require 1000 observations at the least, with an instrument as good as the transit circle, to obtain a difference of parallactic inequality with a probable error of $0''.2$. And, moreover, these 1000 observations must be carefully worked up without unnecessary waste in the working. Now Professor Turner has chosen the altazimuth as his comparison instrument ; he has thrown aside the observations of what he calls "day 15" (or full moon) and smoothed out days 14 and 16. These are the most important observations of all, because the parallactic inequality is then most different from its mean value, and we are at once left with an amount of material insufficient for its purpose. It was not necessary to confine the investigation to fifteen years, and astronomers, like lawyers, ought to adopt the doctrine of "best evidence."

This being my view about a comparison with fifteen years of the altazimuth, *à fortiori* I can give no credit to a result based upon four years, 1847–1850. To my judgment the figures given by Professor Turner (p. 411, Table V., column 2, $0''\cdot0$, $-1''\cdot1$, $+1''\cdot1$, $-0''\cdot2$, $-1''\cdot1$, $-0''\cdot7$, $-0''\cdot2$, $+0''\cdot7$, $+0''\cdot1$, $-0''\cdot5$) are palpably of an accidental character. He implicitly admits this himself of the two largest by smoothing them out, and I was astonished that he should have considered the smaller figures unimpeachable.

In his second paper (May 1904) Professor Turner seems in some apprehension as to whether I have mistaken a cosine term for a sine term. I can assure him he need not be in the least anxiety. His argument is based upon the assumption that we only have one quarter available; an assumption that seems to owe its origin to the fact that in his first paper he quite properly subtracted the results for any day before full moon from the corresponding day after full moon, as he was then dealing with an odd function of D. When it is a question of cosines the corresponding results should be added. If the table in which he has set out so clearly the similarity between sin D and $1+\cos$ D in the second quarter of the Moon (D is usually measured from new moon) be extended to the third quarter, it will be seen that the two functions are of opposite signs. Analytically the cross term in the normal equations, when the errors are equated to $\delta_1 \sin D + \Delta_1 \cos D$, is proportional to the mean value of sin 2D, the value of which is found on p. 94 to be $-0\cdot04$, and has been neglected throughout by me.

It was so obvious to me that no preliminary remarks would avoid the necessity of resorting to normal equations that I omitted the remarks and went straight to the equations. Perhaps now, though somewhat late, I had better give the remarks. I do so in a perfectly general form.

Let ϕ be a function whose coefficient a we wish to determine. Let χ_1, χ_2, &c., be other functions, such that they are liable to be confounded with ϕ. To give a precise meaning to this, consider the method of finding a. The errors are grouped according to the values of ϕ, which we will call ϕ_1, ϕ_2, &c. For the typical group of n_r observations we have

$$n_r a\phi_r = \Sigma e;$$

and its contribution to the normal equation is

$$an_r\phi_r^2 = \Sigma e \,.\, \phi_r;$$

and the complete normal equation is

$$a\Sigma n_r\phi_r^2 = \Sigma e \,.\, \phi.$$

Now a function χ is liable to be confounded with ϕ if

$$\Sigma\chi\phi \neq 0$$

and it will be seen that this is a reciprocal relationship; if χ may be confounded with ϕ, so may ϕ with χ.

Having, then, chosen the function ϕ, whose coefficient I wish to investigate, I pick out all possible forms of the function χ, and the value of my analysis will depend upon the completeness with which I have picked out these functions. In particular—

when $\phi = \sin D$ we have $\chi_1 = \mu$ $\chi_2 = \sin 2D$ (p. 94)
when $\phi = \cos D$ we have $\chi_1 = 1$ $\chi_2 = \cos 2D$ (p. 582)
when $\phi = \sin A$ we have $\chi_1 = \sin (A+D)$ $\chi_2 = \sin (A-D)$ (p. 413)

this last illustration being, as far as I am aware, original; and it, moreover, introduces an important correction (0″·27) into the value of the eccentricity when investigated by comparison with Hansen's Tables (see p. 418). On pp. 420 and 693 I have pointed out cases where it would be necessary to resort to the same process when only fifty years' observations are under discussion, but when the process can be avoided by waiting for the reduction of the Airy period, 1750 to 1851.

For simplicity I now consider one fraction χ only. The disentanglement of ϕ from χ involves of course the disentanglement of χ from ϕ. The errors are therefore equated to

$$a\phi + b\chi.$$

Now if $\Sigma\phi\chi = k\Sigma\phi^2$, it follows that a term $\beta\chi$ is liable in an analysis for ϕ alone to be mistaken for $\beta k\phi$. I therefore in the first place solve for $a+kb$ on the supposition that this is the coefficient of ϕ. Next I try to distinguish between

$$a\phi + b\chi \quad \text{and} \quad (a+kb)\phi$$

that is to say, I try from the errors to measure b considered as the coefficient of $\chi - k\phi$.

Now in most cases it is natural to take for the forms of χ and ϕ functions that vary between the limits ± 1. When ϕ, χ are liable to be confounded, it will generally happen that $\chi - k\phi$ varies between smaller limits, $\pm\lambda$ say. Writing, then, $b\lambda \frac{\chi - k\phi}{\lambda}$ instead of $b(\chi - k\phi)$, so that $\frac{\chi - k\phi}{\lambda}$ varies between the limits ± 1, it will be seen that $b\lambda$ can be determined with approximately the same accuracy as the coefficient of any sine or cosine term that does not present any special difficulty. Also it will be seen that, when it comes to fitting the formula found, in its form

$$(a+bk)\phi + b\lambda \frac{\chi - k\phi}{\lambda}$$

on to the errors, the usual degree of precision may be expected.

It is only when we exhibit the values of a and b separately, for the latter multiplying $b\lambda$ by $\frac{1}{\lambda}$, that we multiply discordances by $\frac{1}{\lambda}$ and get values with weights reduced in the ratio λ^2 to 1.

If we examine Professor Turner's first paper in the light of these remarks, it appears that there has been no adequate attempt to disentangle the quantities I call μ and δ_1. Professor Turner evidently recognises that δ_1 cannot be determined without determining μ; he omits, however, to see that the relationship is reciprocal and that μ cannot be determined apart from δ_1. His method of determining δ_1 is therefore based upon giving two alternative guesses at μ (p. 407); and it will be seen that these alternative values of μ are in reality values not of μ but of $\mu+0{\cdot}7\ \delta_1$ and of $\mu+0{\cdot}4\ \delta_1$ respectively.

I come now to the calculations that I have performed in consequence of Professor Newcomb's suggestion that the daylight observations should be omitted.

I have already stated that it thereupon became necessary to omit the six o'clock winter observations. It is for this reason. Suppose we took the December and January observations only. For these months g' is nearly zero. We are therefore unable to distinguish between

$$\sin (g-g'+\omega-\omega') \text{ or } \sin D$$

the parallactic inequality and

$$\sin (g+\omega-\omega')$$

Now on p. 419 I obtained corrections

$$-0''{\cdot}57 \sin (g+\omega-\omega')-0''{\cdot}46 \cos (g+\omega-\omega')$$

and although I then stated that the arguments were uncertain, and might be $g+\varpi$ or $g+2\varpi+3V-5E$, still the fact remains that as long as errors of this period, or approximately of this period, exist, the parallactic inequality is not to be found from observations in the months of December and January alone. And the same objection applies to any scheme whereby the observations discussed are not symmetrically arranged with regard to the argument g' or time of year.

Consequently I cut down the material to days 10–18 and 39–47 in the notation of my December paper, full moon being at $14\frac{1}{4}$ and $42\frac{3}{4}$ days approximately, so that the observations extend for four days on each side of full moon, approximately.

With the help of Table III. pp. 90–95 *Monthly Notices*, Dec. 1903 the coefficients of the normal equations can be formed on the assumption that every error is equated to

$$\mu+\delta_1 \sin D+\delta_2 \sin 2D.$$

The left-hand sides are

$$\begin{aligned} &3052\mu + 1344\delta_1 - 2097\delta_2 \\ &1344\mu + 768\delta_1 - 1144\delta_2 \\ -&2097\mu - 1144\delta_1 + 1734\delta_2 \end{aligned}$$

Consider the quadric

$$a\mu^2 + b\delta_1^2 + c\delta_2^2 + 2f\delta_1\delta_2 + 2g\delta_2\mu + 2h\mu\delta_1$$

where

$$\begin{aligned} a &= 3052, \quad b = 768, \quad c = 1734 \\ f &= -1144, \quad g = -2097, \quad h = 1344 \end{aligned}$$

The quadric may be put into the form

$$a\left\{\mu + \frac{h}{a}\delta_1 + \frac{g}{a}\delta_2\right\}^2 + \frac{ab-h^2}{a}\left\{\delta_1 - \frac{gh-af}{ab-h^2}\delta_2\right\}^2 + \frac{\Delta}{ab-h^2}\delta_2^2$$

where $\Delta = abc + 2fgh - af^2 - bg^2 - ch^2 = 9152000$

$$\begin{aligned} ab - h^2 &= 537600 \\ gh - af &= 673120 \end{aligned}$$

Consequently the quadric becomes

$$\begin{aligned} &3052\{\mu + 0{\cdot}44\delta_1 - 0{\cdot}68\delta_2\}^2 \\ +\ &176\{\delta_1 - \tfrac{5}{4}\delta_2\}^2 \\ +\ &17\delta_2^2 \end{aligned}$$

and therefore
$\mu + 0{\cdot}44\delta_1 - 0{\cdot}68\delta_2$ can be determined with a weight equal to 3052 single observations

$$\begin{aligned} &\delta_1 - \tfrac{5}{4}\delta_2 \text{ with a weight of } 176 \\ &\delta_2 \text{ with a weight of } 17. \end{aligned}$$

This is practically equivalent to saying that δ_2 cannot be determined at all from the material.

The same results follow, with approximate accuracy, from geometrical reasoning, as follows :—

By changing the sign of all second limb errors, we get a determination, with weight 3052, the total number of observations employed of

$$\begin{aligned} &\mu + \delta_1 \times (\text{mean numerical value of sin } D = 0{\cdot}44) \\ &+ \delta_2 \times (\text{mean numerical value of sin } 2D = -0{\cdot}68) \end{aligned}$$

Next, if we try to measure the difference between

$$\mu+\delta_1 \sin D+\delta_2 \sin 2D$$

and

$$\mu+\delta_1\times 0{\cdot}44-\delta_2\times 0{\cdot}68$$

we find that we are approximately always measuring a multiple of $\{\delta_1-\frac{5}{4}\delta_2\}\times 0{\cdot}4$, and that therefore δ_1, δ_2 cannot be separated.

Lastly, as the factor of $(\delta_1-\frac{5}{4}\delta_2)\times$, $0{\cdot}4$ ranges between the values ± 1, the above quantity can be determined from 3052 observations with a weight of $3052\div 3$ approximately, and therefore $\delta_1-\frac{5}{4}\delta_2$ with a weight $(0{\cdot}4)^2\times 3052\div 3 = 162$, the more accurate weight being 176, as stated above.

I have grouped the forty-eight periods of analysis into six groups of eight, the group of eight being taken as a unit in order to get rid of inequalities with argument $\omega-\omega'$, whose period is 8×400 lunar days nearly. The results are

Periods.	$\mu+0{\cdot}44\delta_1-0{\cdot}68\delta_2$.	$\delta_1-\frac{5}{4}\delta_2$.
86–93	$-0''{\cdot}30$	$-0''{\cdot}24$
94–101	$-0{\cdot}40$	$-0{\cdot}34$
102–109	$+0{\cdot}13$	$-0{\cdot}48$
110–117	$+0{\cdot}29$	$-0{\cdot}20$
118–125	$+0{\cdot}18$	$-0{\cdot}10$
126–133	$-0{\cdot}26$	$-0{\cdot}20$
Means	$-0{\cdot}06$	$-0{\cdot}26$

Considering the superior weight of $\mu+0{\cdot}44\delta_1-0{\cdot}68\delta_2$, the discordance of the separate results is striking, and indicates marked changes in semi-diameter. On the other hand, the accordance of the six separate values of $\delta_1-\frac{5}{4}\delta_2$ is remarkable, for I expected a probable accidental error in each value of $\pm 0''{\cdot}25.(\pm 1''{\cdot}7\div\sqrt{176}\div 6)$. This is perhaps evidence of the superior character of the night observations.

From the mean we have

$$\delta_1-\tfrac{5}{4}\delta_2 = -0''{\cdot}26$$

and to obtain δ_1 we must adopt a value for δ_2 from other sources —with this inconvenience, that any error in δ_2 introduces a larger error into δ_1.

The truth probably lies between the following two extremes :—

(1) The variation should have its theoretical value or $\delta_2 = +0''{\cdot}39$ leading to $\delta_1 = +0''{\cdot}23$.

(2) The variation should have its value as determined from all the observations, or $\delta_2 = -0''{\cdot}03$ and $\delta_1 = -0''{\cdot}30$,

or, in other words, I assume that the variation apparent from the

observations between 8^h and 16^h will be intermediate between the true variation and the variation apparent from all the observations.

Consequently

$$\delta_1 = -0''.03 \pm 0''.27$$

the $\pm 0''.27$ indicating the probable *extreme* limits to the unknown systematic error.

The solar parallax corresponding to $\delta_1 = -0''.03$ is $8''.79$, as found last month.

I take this opportunity of stating that my principal object is to analyse the Moon's errors with a view to obtaining empirical coefficients to be compared with Professor Brown's theoretical ones. From time to time there will be digressions into the values of astronomical constants, accompanied, I hope, by a discussion of the accidental error and a notification of undetermined systematic errors.

Reduction of 295 Photographs of Eros made at Nine Observatories during the period 1900 November 7–15, with a determination of the Solar Parallax. By Arthur R. Hinks, M.A.

§ 1. *Introduction.*—In two previous papers (*Monthly Notices*, 1901 November and 1902 June, vol. lxii. pp. 22 and 551) there is an account of the experimental reduction of certain photographs of *Eros* made at Cambridge, Lick, and Minneapolis. Its object was to test a method of reduction in rectangular coordinates; and the conclusion was that the method was simple, convenient, and worthy of a more extended trial. I therefore ventured to propose that we should undertake at Cambridge the reduction of so much of the photographic material obtained during the period 1900 November 7–15 as might be placed at our disposal by the kindness of the directors of the different observatories taking part in the cooperation to observe the planet *Eros* at that opposition. At the time this proposal was made we had very little information as to the real accuracy of the photographic method when pushed to the limit, and especially little knowledge of what systematic discordances might be found in the work of different observatories. It was hoped that the discussion of the material of these nine days, considerable in itself, but only a small part of the whole, might lead to a preliminary value for the solar parallax of weight equal to that of the best existing values, and at the same time be some guide in the operations which must eventually be undertaken to combine the whole of the observations in one definitive solution.

§ 2. The first step was to choose the stars which were to form the standard of comparison stars.

For this system the *étoiles de repère* selected by M. Loewy for special meridian observation are not suitable. They are chosen for plates 2° square, and in great part lie outside the limits of the smaller fields of many photographic telescopes; and they are on the average considerably brighter than the planet. A set of stars was chosen, including a few of these *repère* stars which lay near the track of the planet, and a number of others which were comparable in brightness with *Eros*, and evenly distributed, so that one might count on getting at least ten suitably distributed stars in any field with a radius of 15′. Photographic copies of a chart of these stars were sent to all the observatories who had photographs within the selected dates, with the request that in measuring their plates they should select from the list about ten stars, with the planet near the centre of gravity of the group, and should send to Cambridge a copy of the measured rectangular coordinates of stars and planet, uncorrected for anything except errors of the measuring machine and réseau.

§ 3. I must make grateful acknowledgment of the kindness with which this request was received. The directors of eight observatories have done me the honour of placing in my hands measures made under their direction, fulfilling in somewhat various ways the proposed conditions.

It will be generally convenient in what follows if each separate set of measures as communicated to me is called a plate, though in most cases a number of exposures were actually made on the same plate. The contributions were as follows :—

Algiers ...	measures of	40 plates
Lick... ...	... ...	28
Minneapolis	... ...	17
Northfield	... ...	23
Oxford (University Obs.)	...	32
Paris ...	... ...	21 (means of 3)
San Fernando	... ...	9
Tacubaya ...	... ...	15

These, added to

Cambridge ...	 110

make up the total of 295 discussed in the present paper.

Some more Oxford plates arrived unfortunately just too late to be included.

Each Paris plate is the mean of three separate exposures. On it are measured, in accordance with the Paris programme, all the *étoiles de repère*, and all the stars lying in a square of 20′ with the planet at its centre. A number of my standard comparison stars are found in this square, and some others are

among the fainter *repère* stars. The average number of standard comparison stars on the Paris plates was 14·5.

The Algiers plates are measured on the same plan, but the exposures are single. The number of standard comparison stars measured on the Algiers plates was on the average 12.

On the San Fernando plates all the *repère* stars and all the stars in my list were measured. On the Tacubaya plates a large number of the stars in my list were measured. On the Lick, Minneapolis, Northfield, Oxford, and Cambridge plates groups of about ten stars were chosen from this list and measured.

The Algiers, Paris, and San Fernando plates are impressed with a réseau, but measured in millimetres ; the Oxford, Cambridge, and Tacubaya plates are measured in réseau intervals (5 mm.) ; the Lick, Minneapolis, and Northfield plates are measured in millimetres without a réseau. The Tacubaya plates were measured by estimation on an eyepiece scale, and the results will be comparatively rough.

§ 4. *Standard Centre.*—A feature of the method to be used is the transformation of every plate to the standard centre and standard axes of rectangular coordinates. The standard centre used in the experimental reductions was in R.A. $1^h\ 57^m\ 8^s{\cdot}0$, Decl. $+54°\ 22'\ 0''$ (1900·0), and it has been found convenient to retain this throughout. The standard axes in the tangent plane to the sphere at this point are respectively at right angles to and along the projection of the meridian through the standard centre.

§ 5. *Construction of the Standard System of Comparison Stars.*—In the Paris circulars Nos. 8 and 9 the results of the meridian observations of the *repère* stars made at a number of observatories are given in the form of a table of mean places found by each observatory. As the number of observations at different observatories varied very much it seemed well to weight the results by the square root of the number of observations on which each depends.

It should be noticed that the system of weights adopted is by no means unimportant. I have remarked in the *Observatory* (vol. xxvi. p. 342) that the final places of the *repère* stars adopted at Bordeaux, Paris, and Cambridge differ systematically from one another, and that in consequence *absolute* places determined by these three observatories would not be homogeneous, and could not properly be used as they stand in a determination of the solar parallax. As, however, I have ventured to contend that it is a mistake to attempt to deduce absolute places of the planet for use in the parallax equations, and shall throughout this paper consider the deduced places of the planet as relative only to my adopted system of stars, it is not necessary to insist further on this point.

Rectangular coordinates for the standard centre and axes were calculated from the adopted places of the *repère* stars for 1900·0, and these will, as usual, be called standard coordinates.

The places of the standard comparison stars were obtained from Paris and Cambridge plates. The Paris plates contained a number of the standard stars in the 20′ square ; and a set of Cambridge plates was specially measured with all the available *repère* stars. These measures, referred to many different centres, were all transformed approximately to the standard centre and axes by the method given in § 8.

The six constants of the ordinary linear reduction were determined from the *repère* stars on each selected Paris and Cambridge plate, and the comparison stars were thus reduced to standard. On collecting the results it appeared that the places deduced from the two series of plates agreed fairly well with one another. The Cambridge x coordinates averaged $0''\cdot1$ greater than the corresponding Paris places, which is probably an effect of the nature of magnitude equation due to the fact that the Paris plates were reduced with many more bright *repère* stars than the Cambridge plates ; it has been shown that there is a sensible magnitude equation in the meridian places (F. Cohn, *A. N.* 3952). But when the differences Cambridge minus Paris were classified with respect to magnitude there was little if any trace of relative magnitude equation between the two. It is possible, therefore, that the zero of the adopted places of the standard stars, the simple mean of all the individual results, may be affected by magnitude equation, but the internal smoothness of the system is unaffected by it. The number of places included in each mean averaged 6·5. It was estimated that the probable divergence of any adopted place from a smooth system (not necessarily from its absolute place) would be less than $0''\cdot1$; and it will be shown later that the internal errors of the system are actually a good deal smaller than was estimated (see § 10).

The resulting catalogue of standard places of forty-eight comparison stars has served for the complete reduction to standard of all the plates (with the exception of one series which will be mentioned later).

§ 6. *Choice of Scale Unit and Reduction to Scale.*—The greater part of the measures sent to me were expressed in millimetres on the plate, the remainder in terms of the 5 mm. réseau. As the focal lengths of the telescopes employed varied from 9 to 20 feet it was necessary to adopt some standard scale to which everything should be reduced. As proposed in the second paper, I have converted all the measures into a scale whose unit is the one-thousandth part of the focal length of the telescopes employed respectively in making the photographs. One unit in the fourth place of decimals is then very nearly $0''\cdot02$; the probable error of a single measure on the best plates is about three times this quantity, so that my unit is just about the right size to give a suitable degree of accuracy in the computations, without any superfluous figures, if the reductions of the stars are carried out to four places of decimals, and the reductions of the measures of the planet only to five.

The first step in the treatment of all the measures was to reduce them approximately to this standard scale of one-thousandth the radius of projection, by multiplication on the arithmometer.

§ 7. *Second Order Terms in the Differential Refraction.*—The next step was to apply to the measures such part of the correction for differential refraction as involves the squares of the coordinates on the plate. These corrections rarely amount to more than a few units in the fourth place; but since they consist of the sum of six small terms they are troublesome to compute. To save this labour I have devised a graphical method of determining them, which is described in *Monthly Notices*, lxiii. 138, 1903 Jan. This method has been used throughout, and has been found very convenient.

§ 8. *Transformation to the Standard Centre.*—The next step is to transform the measured coordinates to the standard centre.

Let the R.A. and Decl. of the original centre of the plate be A_1, D_1; and of the standard centre be A_2, D_2; and let $A_2-A_1=a$.

Then if x_1, y_1 be the measured coordinates of a star, and x_2, y_2 those coordinates transformed to the standard centre, it is easy to show that

$$x_2 = \{x_1 . \cos a + y_1 . \sin D_1 \sin a - \cos D_1 \sin a\} / N$$

$$y_2 = \{-x_1 . \sin D_2 \sin a + y_1 (\cos D_1 \cos D_2 + \sin D_1 \sin D_2 \cos a) + \sin D_1 \cos D_2 - \cos D_1 \sin D_2 \cos a\} / N$$

where the denominator N is equal to

$$x_1 . \cos D_2 \sin a + y_1 (\cos D_1 \sin D_2 - \cos D_2 \sin D_1 \cos a) + \sin D_1 \sin D_2 + \cos D_1 \cos D_2 \cos a.$$

A rigorous transformation by these formulæ is exceedingly cumbersome; it is, however, unnecessary. The transformation differs from a linear transformation owing to the presence of the denominator, and consequent introduction of terms of the second order in x_1 and y_1. But when the distance between centres amounts to only a few degrees these terms are small; and when the distance is only a few minutes they are negligible.

Consider the following equations:

$$M x_2 = (x_1 + B y_1 + C)(1 - K x_1 - L y_1)$$
$$M y_2 = (-B x_1 + y_1 + D)(1 - K x_1 - L y_1)$$

where $B = \sin D_2 \sin a$.

C and D are approximate mean values of x_2-x_1 and y_2-y_1, obtained by inspection of the measured and standard coordinates.

$$K = \cos D_2 \sin a / M.$$
$$L = (\cos D_1 \sin D_2 - \sin D_1 \cos D_2 \cos a)/M$$

and $$M = \sin D_1 \sin D_2 + \cos D_1 \cos D_2 \cos a.$$

For a range of centering such as I have used M never differs from unity by more than 0·0005. We can neglect it in the calculation of K and L, and need not trouble to multiply x_2 and y_2 by it, since the small change of scale value introduced will be eliminated in the further reductions. The numerators differ slightly from the rigorous expressions, but are sufficiently close to it to give the terms of the second order. If, therefore, we transform by these approximate relations, omitting M, instead of by the exact expressions given before, we introduce the second order terms accurately, but leave outstanding some not very large terms of the first order, which are included in the linear reduction which follows.

When our unit is the one-thousandth of the radius of projection, and $D_2 = 54° 22'$, we have

$$B = a \times 0{\cdot}000\,236$$

$$K = a \times 0{\cdot}000\,000\,169$$

$$L = (D_2 - D_1)' \times 0{\cdot}000\,000\,291 + a^2 \times 0{\cdot}000\,000\,000\,020,$$

where a is $(A_2 - A_1)$ expressed in minutes of arc, and K, L have been adjusted to the unit in which the x's and y's are expressed.

This method of approximate transformation of centres has been used throughout, and has proved very simple and convenient. When the plates are in pairs—that is, two exposures close together on one actual plate—these computations are easily checked by differencing.

§ 9. *Reduction to Standard.*—The six constants of the linear formulæ for reduction to standard were determined for each plate in the usual way by the method proposed by Dyson. When there are many plates with the same group of comparison stars it saves work to invert the process and reduce standard to observed; only the numerical terms are then different from plate to plate. Afterwards the constants thus found must be reversed, and when their values are large the reversal involves a little more than mere change of sign. It is easily shown that if a, b, c, d, e, f are the constants for reduction of standard to observed, and A, B, C, D, E, F the constants for reduction of observed to standard, then

$$A = -a + a^2 + bd \qquad D = -d + ad + de$$

$$B = -b + ab + be \qquad E = -e + bd + e^2$$

$$C = -c + ac + bf \qquad F = -f + cd + ef$$

These expressions were used in a paper by the author in the *Astronomical Journal*, No. 475 (vol. xx. p. 151); but it should be noted that a misprint occurs there of c for e in two places.

All the comparison stars as well as the planet have been reduced to standard, and the residuals formed from the adopted standard places. The condition that the sum of the residuals in each coordinate should be zero is a valuable final check on a reduction of which the greater part has already automatically checked itself.

§ 10. *Accuracy of the Adopted Standard System of Comparison Stars.*—The residuals in the reduction of each star, in the sense standard minus observed, may be considered as apparent corrections to the adopted standard places. They have been collected from the Cambridge, Lick, and Paris reduction sheets, and an apparent mean correction formed for each star that had been used more than six times. The mean of all, without regard to sign, was

$0''{\cdot}04$ in x $\qquad$ $0''{\cdot}05$ in y

Of course these individual apparent corrections are really corrections relative to the mean of the group of about ten stars used in the reduction of the plate, so that they will average a little less than the corrections relative to the mean of the whole system. But the groups are thoroughly interwoven and overlapped, and the uniform character of the residuals is a sufficient guarantee against any sudden discontinuities in the system, which alone could do serious harm.

Moreover the residuals used in the above discussion are not altogether free from the effects of certain systematic errors which will be discussed later, and whose elimination would have some tendency to reduce discordances. I think that we may conclude that the P.E. of an adopted standard place is at least as small as $\pm 0''{\cdot}04$, and that the effect of errors in the standard places on the resulting places of the planet will be very small.

§ 11. *Examination of the Series of Photographs for Systematic Error.*—If it had been possible for each observatory to secure observations of *Eros* symmetrically disposed with regard to the meridian there would have been comparatively little reason to fear that systematic errors running through a series of photographs would prejudicially affect the value of the solar parallax deduced from them. But it was actually the case that bad weather made large and unsymmetrical gaps in the work of all observatories. Moreover some observatories have a large preponderance of evening observations in the days about opposition, and all have of necessity a great excess of post-meridian observations throughout the period some weeks after opposition, when the planet was nearest and its parallax greatest. This inevitable dissymmetry gives every opportunity to systematic errors to exert a baneful effect, and it seems to me not possible to accept as axiomatic the statement, which has been made more than once, that they may be trusted to eliminate one another completely in the combination of a large series of observations. On the contrary, two of the series of photographs which we shall

have to discuss contain systematic errors so large that they would ruin any determination of the solar parallax into which they were introduced. The errors can, however, be detected by an examination of the residuals in the comparison star reductions. Our next step will be to show that a critical examination of the residuals in each series of plates must be made an essential part of any discussion of the solar parallax from photographs.

It will be convenient to summarise the kinds of error to which attention has at various times been directed.

Errors of Measurement.—All experience goes to show that these are relatively unimportant. The Cambridge machine has no sensible errors of scale or screw, and it will be shown later that the réseau is nearly perfect. The evidence for freedom from error of other machines whose results enter here is not complete, but it may be assumed that there are no large errors. There seems good reason to believe that personality in measurement is very nearly eliminated by reversal of the plate, which has been done in all series except one.

Real Errors of the Image.—A large part of the error in a measured coordinate is due to real error in the position of the image. Evidence is accumulating that a set of images close together may be affected by a quite large common error, which is probably local distortion of the gelatine film within the réseau square. When no réseau is used this error may of course be much greater.

Constant optical distortion of the field may be expected in reflector photographs, but not otherwise. There seems good reason to suppose *a priori* that it should be symmetrical about the centre of the field, but it will evidently depend very much upon the brightness of the stars.

Hour-angle Error.—Under this name Kapteyn conveniently includes the effect of atmospheric dispersion, and also optical distortions and tilt of plate, varying with the hour angle, due to flexure of objectives, mirrors, and mountings.

Magnitude Equation and Guiding Error.—If the driving is not perfect, but there is a tendency to accumulate error in one direction until it is corrected by hand or automatic control, the brighter stars may be displaced relatively to the faint, and show a kind of magnitude equation varying from plate to plate, but on any one plate depending on the magnitude of the star alone. This is probably one of the most important and subtle errors of the photographic method. It is distinguished from other kinds of magnitude equation, due to an imperfectly symmetrical diffraction pattern of the image, or to personality of measurement, by the fact that its appearance will be capricious.

We shall have occasion to deal with most of these sources of error in the discussion that follows.

A. *The Lick Plates.*—The Lick plates were taken with the Crossley reflector of 36-inch aperture and 17·6 feet focal length, with short exposures averaging 15^s. They were measured on

the Repsold machine at Columbia University Observatory, N.Y. Upon reduction to standard the comparison stars on many of the plates had unduly large residuals, amounting in some cases to a second of arc. There was plenty of evidence from other sources that the standard places were of a high order of accuracy, and it became clear that the error was to be found in the plates or the measures. It was chiefly in the x coordinates, appearing in this way. The x residuals, in the sense adopted standard minus reduced measure, were positive for stars near the middle region in x, and negative for stars towards both sides, the sum of the residuals being necessarily zero. This arrangement of signs indicated an apparent bodily displacement of the central images relative to the outer, and there was consequently every reason to fear that the positions of the planet deduced from the plates might be systematically wrong. The effect was not constant ; on a few plates it seemed to be absent, and in one or two cases changed sign. It had no apparent relation to the hour angle ; was almost exactly the same for a pair of exposures on the same plate, but varied capriciously from plate to plate.

In general character it resembled the effect of a tilt of the plate relative to the optical axis, such as might be caused by an error in squaring on the plate, or by defective collimation of the mirrors. But it needed only a small computation of the amount of tilt required to explain the magnitude of the error, to show that this explanation was grotesque : a tilt of several degrees is entirely out of the question.

The asymmetry of the error with respect to the centre seemed to make it improbable that it was due to optical distortion.

In order to elucidate this perplexing question the Director of the Columbia University Observatory very kindly had measures made of a large number of stars on certain overlapping plates, and the reduction of these confirmed the reality of the error without throwing any light on its cause. I then asked Professor Campbell to be so good as to let me examine some of the plates at Cambridge, and he very kindly sent me three of them. Inspection showed that there was no possibility of a large tilt of the plate ; the optical centre was strongly marked by the perfection of the images around it and their regular degradation as they departed from it. It appeared to me, however, that many stars had been measured which I should myself have judged unfit for measurement on account of the familiar umbrella-shaped character of the images, so I decided to try rejecting all the measures of stars more than 20 mm. or 13′ from the centre—that is to say, all stars whose discs were not sensibly round. The effect of doing so was remarkable. When a new solution was made with the stars that were left the residuals were quite small, and the resulting place of the planet was very much altered ; and when the outer rejected stars were reduced to standard with the constants derived from the inner stars they gave residuals up to $1''.5$.

It is clear that the error was due to the use of stars too far from the centre, and that it is unsafe to measure the Crossley plates where the star discs are not sensibly round. But it remains a mystery why the error should take the shape it does. The images are distorted quite symmetrically with respect to the centre, and there seems no reason why the observer should make an error of measurement always more or less in the same direction on images oppositely distorted. I have tried to go further into this matter, and to discover something more definite about the law of the error, but have failed. This is, perhaps, not surprising when one considers that the shape of the distorted image depends very much on the magnitude of the star as well as on its position. It is unlikely that one will ever be able in such a complicated case to devise a system of corrections applicable to measures of distorted discs, and the safe course will be to confine measurement of these plates to a field of about 12′ radius, within which the images are small and beautifully defined.

The places of *Eros* to be used in the present reductions depend entirely on these central stars.

B. *Algiers Plates.*—A very puzzling error exists in the Algiers plates, which are taken with the standard Astrographic Refractor. I had hoped to be able to use the Algiers measures in forming the system of standard stars, for many of those stars were to be found in the 20′ square about the planet. But when they were reduced to standard with the aid of the *repère* stars in the usual way they were systematically discordant from the results of the Cambridge and Paris plates, and could not be used.

At first sight the error looked like a magnitude equation making the x's of faint stars too large ; but I am now convinced that magnitude equation is not the chief error. It would be hard to go into details without taking up too much space ; the question is much complicated by the fact that the stars measured near the centre of the plate are mostly faint, and the stars further out most of them brighter ; they give respectively negative and positive residuals. To examine this question properly it would be necessary to measure both bright and faint stars distributed uniformly over the plate. But I have found as a rule that a bright star among faint ones near the centre gives a negative residual very much like theirs, while a faint *repère* star far out gives a positive residual as large as that for neighbouring brighter stars. Further, if very few stars in the centre of the field have been measured at all, the residuals immediately become small, and there is no marked magnitude equation between bright and faint outer stars. The conclusion seems to be that the central stars are displaced upon the plate, not because they are faint, but because they are central ; and it was entirely consistent with this to find that Algiers places of the planet differed systematically from the Paris and Cambridge places by quantities of the order of half a second of arc.

This is very remarkable. One is accustomed to expect certain difficulties with a reflector; but it is disquieting to find that a standard astrographic refractor, when devoted to solar parallax work after long use on the astrographic chart and catalogue, can give results which are wrong by amounts quite serious when judged from the standard of the catalogue, and hopeless as they stand for parallax work. It is not easy to imagine what may be the cause. We cannot suppose that the plates were tilted enough to explain it, and we have not material enough to find a general expression of the second degree to represent it.

It is hard to decide what to do in a case like this. If the error is like the error in the Lick plates, and develops suddenly as one gets away from the centre, it is probably safe to rely on the central stars to give a good reduction. This appears to be the case. Having decided to reject altogether the stars outside the 20′ square, I chose some additional standard stars, and got their places as before, measured them on the Algiers plates, and so had always at least six stars for the reduction, except in a few cases where scarcely any central stars appeared on the plates; these were rejected.

The new places of the planet differed from the old by amounts up to 0″·65 and averaging 0″·48. They seemed to fit the ephemeris very well. But it must be admitted that this is our only ground for assuming that the error for whose existence we can assign no reasonable cause has been altogether eliminated.

C. *Cambridge Plates. Search for Réseau Errors.*—On the Cambridge plates the planet is not always referred to precisely the same réseau line, whose error, if any, might appear to give a uniform correction to the place; nor is it referred to so many different lines that accidental errors might be expected to go out in the long run. The réseau must, therefore, be examined. Several published investigations of Gautier réseaux show that the errors of the réseaux themselves are very small, of the order of the division errors in the best divided scales. But it has recently been shown by Ludendorf (*A. N.* 3746) and Bohlin (*Astr. Iakktagelser*, Stockholm, Bd. 6, No. 5) that a réseau perfect in itself may produce imperfect copies through what is known as projection error. It seems that something—probably the fine cut in the glass made by the diamond point that cuts away the silver—may deviate the light just as it passes through the réseau, and produce errors in the copy much larger than in the original. If we determine the errors of the original we must also determine the projection errors, and both are difficult and troublesome matters. We may, on the other hand, achieve our end, which is to find the errors of the photographic copy (apart from gelatine distortion), by examining plates impressed with a réseau to a number sufficiently large to eliminate the gelatine distortion, provided that this may be treated as accidental, and vanishing in the mean of a large number of plates. The supposition is

most likely correct for the central portions, and incorrect for the edges of the plate ; it will serve our present purpose.

I am indebted to Mr. Russell for the use of a strip of photographed réseau, of which he has determined the errors, so that it can be used as a standard scale. This was superposed on several plates, parallel to the X axis, so as to almost coincide with the réseau lines at the points from which the x coordinates of the planet are measured. The distances between eight or ten réseau lines on these plates and adjacent lines of the standard scale were measured, the corrections for errors of the scale were applied, and the resulting apparent errors of the photographed réseau in sixteen strips altogether were plotted. The results are briefly as follows :

The apparent errors of successive lines deviate from a smooth and nearly straight line by quantities averaging about 0·001 mm., equivalent to $0''{\cdot}035$ on my plates ; a considerable part of this deviation must be accidental error of measurement. The errors of two points of a line not more than 1 mm. apart are frequently of opposite sign—that is, the line images are a little rough. When means are taken the deviations of the apparent mean errors from uniformity are almost within the limits of probable accidental error. The conclusion is that there is practically no evidence of sensible error in the photographed copies of the réseau, apart from the general smooth distortions of the gelatine film which the use of the réseau eliminates. In one case only did an error amount to 0·005 mm., and at that point the réseau line was obviously defective ; the defect had been noted when the plate was measured, and special precautions taken to avoid it.

Search for Hour Angle Error.—It is clear that errors of this category can produce no resulting error in the place of the planet unless they are differential, varying from star to star on the plate. If they are due to dispersion they will bear a definite relation to the projected position of the zenith on the plate. If they are due to variable distortion they will probably at least change continuously with the hour angle. In looking for them I plotted the apparent correction to the adopted place of each comparison star deduced from thirteen groups of four exposures each ; and alongside them the relative positions of the projected zenith. There was no relation to the zenith ; nor was there continuity of any kind in the group displacements. I conclude that there is no sign of dispersion or of any differential distortion in the stars on the Cambridge plates. That does not prove that there is no dispersion effect on the planet, but it renders it more unlikely, since there is some experimental evidence to show that the effective wave-length of the planet's light is in no way abnormal (Prosper Henry, *Paris Circular*, No. 8, p. 41).

Guiding Error.—In searching for guiding error one naturally looks for large residuals for the few bright stars, balanced by smaller residuals of opposite sign for the more numerous faint stars. It must, however, be borne in mind that an uneven dis-

tribution of the bright stars may to a large extent mask this effect. If we have two or three bright stars at one edge of the group, and no accompanying faint stars, the linear reduction may strain the fit to suit these bright stars at the edge, and leave them with small residuals. The existence of large residuals for certain bright stars, especially if near the centre, is therefore an indication of guiding error which is not necessarily contradicted by the fact that certain other bright stars have not large residuals of the same sign.

On examination of the residuals for the Cambridge plates I was struck with the fact that the average residual of two bright central stars was large on twelve consecutive plates (1900 November 10), while the residuals for two bright stars at the edge of the group were small. This looked like a case of guiding error, and I was fortunately able to make a more than usually conclusive test. My earlier experimental reduction of this plate depended on faint stars. A comparison of the numerical terms in the equations of condition of this first reduction with the corresponding terms in the present solution ought to show, perhaps, a steadily increasing difference, but no irregularities. This was so, except for the twelve exposures under suspicion, which showed marked divergences of about $0''{\cdot}2$, and strengthened the idea that the bright stars were in error and spoiling the reduction. I therefore rejected these four stars and used four fainter, making a new solution which gave decidedly different values of the constants, and altered the place of the planet by about $0''{\cdot}1$ on an average.

No other plates show signs of guiding error comparable with this found on the bad set of twelve consecutive plates, which may perhaps be attributed to some temporary derangement of the automatic control. But the possibility of so large an error affecting a series of exposures with large parallax factors of the same sign is one that must be reckoned with, and seems to me a most powerful argument for the necessity of examining the residuals of all the comparison stars on every plate.

D. *San Fernando Plates.*—The result of the reduction of the San Fernando plates is disappointing. Admiral Viniegra very kindly had measured for me the places of all my selected comparison stars, in order to give additional material for the formation of my standard system; but when the reductions were made the results differed so largely and irregularly from the accordant results of Paris and Cambridge that it seemed unwise to use them in the formation of the standard system. On inquiry I learned that the plates had been measured in only one orientation. This fact compels me, very regretfully, to exclude the San Fernando results from my solution.

E. *Northfield Plates.*—A good many of the Northfield plates were taken on a plan which, one may venture to say, is certainly unsatisfactory. The guiding star was placed successively on the four corners of a square reticle, and then once more on the first

corner. The motion of *Eros* in the interval was sufficient to clear the images of the planet, but the stars in the first corner exposures are superposed and measured as one set. Apart from the fact that the fit of the two sets of star images cannot be perfect, owing to changes in the differential refraction, orientation due to maladjustment of polar axis, &c., there are grave reasons for fearing guiding error when two exposures are superposed with an accuracy limited by the accuracy of pointing with a not very powerful guiding telescope. I think that there can be no doubt that the comparative roughness of the Northfield results is partly due to this procedure. An examination of the comparison star residuals does not, however, show any signs of systematic error on the plates.

F. *Minneapolis Plates.*—An examination of the residuals in the reduction of the comparison stars to standard showed an error which is probably due to some kind of optical distortion. The measurers' notes contain references to elongated and winged images, and it is clear from the Lick results that these must be treated with caution. I should have tried to treat these plates in the same way that the Lick plates were treated had it not been for the fact that on many Minneapolis plates the images of the planet are far from the centre of the plate, and the group of comparison stars is in consequence very unsymmetrically placed upon it. As an experiment I have made new solutions after rejecting the outer stars of the originally selected groups, and have obtained new places of the planet differing systematically from the old. But, owing to the want of symmetry, there does not seem to be any guarantee that these new places are clear of error ; and the danger of using them is aggravated by the fact that the parallax factors for the Minneapolis plates are nearly all of one sign. On taking all these things into account I was very reluctantly compelled to decide that it would be safer to leave out the Minneapolis results from my solution.

The Minneapolis plates also suffer from the fact that a good many of the exposures on the stars are multiple, made at intervals great enough to allow the images of the moving planet to clear one another. The roughness of the accordance between the resulting places of the planet and the ephemeris confirms the impression derived from the Northfield plates that this is a method of procedure which should not be employed again.

G. *Oxford Plates.*—On one of the Oxford plates there is a clear case of guiding error running almost uniformly through half a dozen exposures. It has been treated in the same way as the like Cambridge case, by rejecting the bright stars and solving afresh.

H. *Paris Plates.*—No case of abnormality or suspicion of systematic error has arisen in the examination of the Paris star residuals.

§ 12. *Adopted Weights of the Different Series.*—We have now to adopt a system of weights for the different series of plates,

taking into account both the tendency towards systematic error revealed in some of them and also the general character of the plates as indicated by the average size of the comparison star residuals upon them. The average residuals are :—

	In X.	In Y.	Adopted Weight.
	″	″	
Algiers	0·16	0·12	$\frac{2}{3}$
Cambridge	0·11	0·10	1
Lick	0·10	0·12	1
Minneapolis	0·21	0·20	0
Northfield	0·18	0·17	$\frac{1}{4}$
Oxford	0·17	0·17	$\frac{2}{3}$
Paris	0·10	0·11	1
Tacubaya	0·28	0·19	$\frac{1}{10}$

The eleven Cambridge plates on which guiding error was found have been given weight $\frac{2}{3}$.

Part of the average residual is due to errors in the adopted places of the stars, whose probable error is less than 0″·04. And it will be shown later that the connection between the average star residual on the plate and the final residual in the equations of condition is not so close as might have been expected. After consideration of all the circumstances I have adopted the weights given in the last column of the above table.

§ 13. *Summary of Observations used.*—

Algiers.

40 plates were measured and communicated.
1 was rejected for large discordance, evident in original measures.
8 were rejected for want of central stars.
31 were used.

Cambridge.

110 plates were measured.
5 were rejected for various reasons—wrong time record, clouds during part of exposure, or large discordances for images marked "probably defective" during measurement.
1 was omitted by mistake.
104 were used.

Lick.

28 were measured, and all were used.

Northfield.

23 were measured.
2 were rejected for clouds during exposure.
21 were used.

Oxford.
55 were measured.
25 were too late to be included.
30 were used.

Paris.
21 were measured and all were used.

Tacubaya.
15 were measured.
1 was rejected for large discordance.
14 were used.

The 9 San Fernando and 21 Minneapolis were not included, for reasons given above.

§ 14. *Construction of the Plate Ephemeris.*—A special "plate ephemeris" for my standard centre was constructed in the way described in my second paper (*Monthly Notices*, lxii. 1902, p. 554, § 6), to give standard coordinates of the places of the planet at the times light left it, as seen from the Earth at successive Berlin midnights when the light arrived. It was based on the separate heliocentric ephemerides in ecliptic rectangular coordinates of the Earth and of *Eros*, published by M. Lœwy in *Paris Circular*, No. 8.

These ephemerides have been computed with 8-figure tables, and are given to eight places of decimals. The third differences do not run quite smoothly in either, and I have found it possible to improve them by making small alterations not exceeding three units in the last place of decimals. In the case of the heliocentric ephemeris of *Eros* this is perfectly simple. But in so treating the ephemeris of the Earth one must be careful not to smooth out any part of the real inequality due to the lunar equation. In order to avoid this the third differences in the Earth ephemeris were plotted against the time. The character of the real periodicity in the differences due to the Moon was then quite conspicuous, and all that was done was to make such slight alterations in the last place of decimals as would remove roughnesses without modifying in the least the character of the curves.

Plate Ephemeris of Eros

for Standard Centre α $1^h\ 57^m\ 8^s{\cdot}0$. $\delta + 54°\ 22'\ 0''$ (1900·0).

Berlin, Midnight. 1900.	ξ_0	η_0	Berlin, Midnight. 1900.	ξ_0	η_0
Nov. 5	+25·23477	−1·95306	Nov. 12	−11·30440	−1·74989
„ 6	+19·98262	−1·15023	„ 13	−16·38109	−2·77464
„ 7	+14·72449	−0·60057	„ 14	−21·38732	−4·06752
„ 8	+ 9·46989	−0·30717	„ 15	−26·31072	−5·62885
„ 9	+ 4·22892	−0·27297	„ 16	−31·13868	−7·45847
„ 10	− 0·98781	−0·50062	„ 17	−35·85846	−9·55570
„ 11	− 6·16937	−0·99235			

There seems to be no doubt that this ephemeris is free from accidental roughness to a nicety well within what our observations will demand; and I must repeat the expression of my acknowledgments to M. Lœwy for his kindness in providing the separate heliocentric ephemerides with which it was constructed.

It may be noted here that there was an error in the computation of the plate ephemeris used in my second paper. The terms expressing the small heliocentric latitude of the Earth were taken with the wrong sign, which accounts for a peculiarity of the solution given in *Monthly Notices*, lxii. p. 559.

§ 15. *Interpolation from the Plate Ephemeris and Calculation of Parallax Factors.*—It is a decided advantage of the plate ephemeris that the third differences multiplied by the interpolation coefficients are small and the fourth negligible. It is also most convenient to have the ephemeris expressed in terms of a single quantity, the adopted unit, instead of in hours, minutes, and seconds of time, and degrees, minutes, and seconds of arc; the labour of computation is thereby much reduced.

The geocentric coordinates of the observatories at the epochs of observation were calculated according to the formulæ given in *Monthly Notices*, lxii. p. 30. (But see below.) But it should have been remarked there that it is proper to use the *apparent* R.A. and Dec. of the standard centre, viz. $1^h\ 57^m\ 15^s$, $+54^\circ\ 22'{\cdot}5$, in calculating those coordinates.

In computing the Lick coordinates the altitude of the observatory was taken into account.

The parallactic displacements of the planet were calculated with an assumed value of the solar parallax $\pi = 8''{\cdot}800$.

[It may be noted here that there is an error in my first experimental paper (*Monthly Notices*, lxii. pp. 30 and 31). The equations at the bottom of p. 30 should be printed

$$\xi = \frac{X}{Z} = \frac{I}{N}\left\{L - \left(a - \frac{c\,L}{N}\right)\pi\right\} = \nu\left\{L - (a - c\xi_0)\,\pi\right\}$$

$$\eta = \frac{Y}{Z} = \frac{I}{N}\left\{M - \left(l - \frac{c\,M}{N}\right)\pi\right\} = \nu\left\{M - (b - c\eta_0)\,\pi\right\}$$

and throughout the following page the quantities printed $\nu(a - c\nu)$ and $\nu(b - c\nu)$ should read $\nu(a - c\xi_0)$ and $\nu(b - c\eta_0)$. The mistake arose in preparing the paper for press; the right formulæ were used in the calculations.]

§ 16. *Formation of the Equations of Condition.*—On comparing the observed places of the planet with the ephemeris it was evident that the correction to the x ephemeris was nearly constant throughout, and that the correction to the y ephemeris varied slightly with the time. There were no indications of terms depending on squares and higher powers of the time. Of course it will be necessary to make a careful search for such terms; but what would be, from the parallax point of view, a good deal more important would be the existence of a short

period variation in the place of the planet related to the light variation. This could be detected most conveniently in the residuals from a preliminary solution. I therefore adopted as my first equations of condition a simple form with only three unknowns in each, $\Delta_1\xi_0$ or $\Delta_1\eta_0$, constant corrections to the plate ephemeris, $\Delta_2\xi_0$ or $\Delta_2\eta_0$ the variation of these corrections per day, and $\Delta\pi$ the correction to the assumed parallax.

The equations expressing the comparison between observation and ephemeris have the form

$$\begin{aligned}\xi_0+\Delta_1\xi_0+t\,.\,\Delta_2\xi_0\ &-(\pi+\Delta\pi)\,(a-c\xi_0)/\mathrm{N}=x\\ \eta_0+\Delta_1\eta_0+t\,.\,\Delta_2\eta_0\ &-(\pi+\Delta\pi)\,(b-c\eta_0)/\mathrm{N}=y.\end{aligned}$$

Hence, putting

$$\begin{aligned}\xi_0-\pi(a-c\xi_0)/\mathrm{N}-x&=m'\\ \eta_0-\pi(b-c\eta_0)/\mathrm{N}-\eta&=n',\end{aligned}$$

we have as the form of our equations of condition

$$\begin{aligned}\Delta_1\xi_0+t\,.\,\Delta_2\xi_0-(a-c\xi_0)/\mathrm{N}\,.\,\Delta\pi+m'&=0\\ \Delta_1\eta_0+t\,.\,\Delta_2\eta_0=(b-c\eta_0)/\mathrm{N}\,.\,\Delta\pi+n'&=0.\end{aligned}$$

Working in units of the fifth place of decimals (that is, in hundred-millionths of the radius of projection), we have on the average

$$\begin{aligned}m'&=+1090\\ n'&=-\ 120,\end{aligned}$$

and we shall take as the numerical terms in our equations of condition

$$\begin{aligned}m&=m'-1090\\ n&=n'+\ 120.\end{aligned}$$

§ 17. *Normal Equations.*—The normal equations derived from the measures of each observatory are as follows: the coefficients of $\Delta_1\eta_0$ and $\Delta_2\eta_0$ are the same as those of $\Delta_1\xi_0$ and $\Delta_2\xi_0$, and are not repeated :—

Algiers (*with weight* $\frac{2}{3}$).

$20{\cdot}67\Delta_1\xi_0$ + $48{\cdot}00\Delta_2\xi_0$	+ $1{\cdot}05\Delta\pi$	− 23 = 0	− $1{\cdot}80\Delta\pi$	+ 191 = 0
+ 144·9460	+ 1·1289	+ 24·00	− 1·9751	+ 1541·67
	+ 64·8469	+ 502·17	+ 7·2655	− 138·14

Cambridge (209–252).

39 − 86·62	− 14·77	+ 143	− 22·73	− 3554
+ 203·9652	+ 34·6727	− 648·74	+ 52·9410	+ 8341·17
	+ 69·5633	− 265·00	+ 19·5289	+ 2189·68

Cambridge (254–265, *with weight* $\frac{2}{3}$).

7·33	− 8·68	+ 10·13	− 86	+ 4·09	− 162
	+ 10·2813	− 12·0257	+ 106·15	+ 4·8973	+ 212·92
		+ 14·1997	− 134·35	+ 2·6597	+ 135·04

Cambridge (266–335).

54	+ 64·36	− 15·03	− 1294	− 21·64	+ 896
	+ 320·3484	+ 52·4368	− 2041·52	− 27·6246	+ 7266·53
		+ 78·3565	− 482·45	+ 16·6724	− 347·92

Lick.

28	+ 8·60	+ 25·63	+ 614	+ 5·40	− 553
	+ 91·7948	+ 5·7855	+ 676·45	+ 6·1198	+ 3376·39
		+ 63·2493	+ 1047·91	+ 7·5570	+ 15·22

Northfield (*with weight* $\frac{1}{4}$).

5·25	+ 4·69	+ 5·09	− 180	− 0·47	+ 114
	+ 53·8704	+ 7·4601	− 283·77	− 4·7879	+ 1550·90
		+ 9·6121	− 238·79	+ 1·1716	− 26·30

Oxford (*with weight* $\frac{2}{3}$).

20	+ 1·19	− 4·25	− 691	− 20·14	− 377
	+ 111·9618	+ 41·7571	− 353·11	+ 11·2237	+ 2924·95
		+ 46·1206	− 375·12	+ 24·8782	+ 805·81

Paris.

21	− 25·19	+ 7·28	+ 299	− 3·73	− 561
	+ 202·3461	− 46·8666	+ 666·11	+ 0·1252	+ 3416·05
		+ 24·7698	+ 84·97	+ 7·1409	+ 166·38

Tacubaya (*with weight* $\frac{1}{10}$).

1·40	+ 2·44	+ 0·35	+ 11	+ 1·35	+ 107
	+ 13·7509	− 2·2404	+ 541·80	+ 1·8865	+ 674·97
		+ 3·7134	− 94·33	+ 1·8564	+ 134·49

The normal equations for the combined solution are, therefore :—

$+196{\cdot}65\Delta_1\xi_0$ +	$8{\cdot}79\Delta_2\xi_0$ +	$15{\cdot}48\Delta\pi$	$-1207{\cdot}00=0$	$-67{\cdot}75\Delta\pi$ − 3899 = 0
	+ 1153·26	+ 82·11	− 1312·63	+ 42·81 − 29305·55
		+ 374·43	+ 45·01	+ 8ℰ·73 − 2934·26

And the solution is : From the x equations :—

	Weight.
$\Delta_1\xi_0 = +6{\cdot}136$	196
$\Delta_2\xi_0 = +1{\cdot}136$	1135
$\Delta\pi = -0{\cdot}623$	368

From the y equations :—

$$\Delta_1\eta_0 = +18{\cdot}688 \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad 143$$
$$\Delta_2\eta_0 = -25{\cdot}309 \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad 1121$$
$$\Delta\pi \quad = -\ 6{\cdot}589 \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad 64$$

§ 18. *Relative Weights from the Residuals.*—The following table gives the average residual in an equation of condition weighted according to the scheme given above.

	Weight Adopted.	Average Residual. X Equations.	Y Equations.	No. of Equations.
		″	″	
Algiers	$\frac{2}{3}$	0·109	0·111	31
Cambridge (209–252) ...	1	·152	·129	39
„ (254–265) ...	$\frac{2}{3}$	·132	·080	11
„ (266–335) ...	1	·105	·087	54
Lick	1	·101	·093	28
Northfield	$\frac{1}{4}$	·111	·134	21
Oxford	$\frac{2}{3}$	·134	·105	30
Paris	1	·093	·099	21
Tacubaya	$\frac{1}{10}$	·128	·103	14

The result shows that some improvement might perhaps be effected by further weighting. The first Cambridge series is not good, and should have less weight; so also should the Oxford series; while Paris should have had more weight. This is on the assumption that the residuals are accidentally distributed, which I shall show is not the case. It follows that the above average residuals are not necessarily proportional to the true weights, but they may be taken as showing that the system of weights adopted did not differ widely from the truth. We shall not improve matters by making a fresh solution with revised weights before we have considered the systematic deviations of each series from the mean.

§ 19. *Systematic Deviations of each Series from the Mean.*—In the first column of the following table we have the mean residual (taking account of signs) in the equations of condition from each series of plates :—

	Mean Residual. In X.	In Y.	Parallax Factors in X. Numerical Sum.	Algebraic Sum.
	″	″		
Algiers	+0·013	−0·050	41	+ 1
Cambridge (209–252) ...	+ ·016		50	−15
„ (254–265) ...	− ·013	+ ·004	12	+12
„ (266–335) ...	− ·033		58	−14
Lick	+ ·057	− ·021	34	+26
Northfield	− ·028	+ ·019	13	+10
Oxford	− ·047	+ ·009	37	− 5
Paris	+ ·039	+ ·048	18	+ 7
Tacubaya	+ ·011	+ ·029	6	+ 1

Taking the probable error of an X equation of condition as $\pm 0''\cdot 10$, we see that the third Cambridge series, the Lick and Oxford, and perhaps the Paris series have mean residuals considerably larger than might be expected if they were fortuitous; there is an appearance of systematic divergence from the general mean. It becomes important to see whether this may have affected prejudicially the value found for the parallax. The fourth and fifth columns contain the numerical and algebraical sums of the parallax factors in the X equations of condition. They show whether in any series parallax factors of one sign have predominated. Further, since similarity of sign in the corresponding quantities of the second and fifth columns shows a tendency to diminish the deduced value of the parallax, we are able to judge from this table in what direction a systematic error will have acted.

The most conspicuous case of want of balance in the parallax factors is found in the Lick series (where evening plates were deliberately selected to coincide in time with morning plates of the eastern hemisphere). If the Lick residuals are really systematic this series will have tended to reduce the value of the parallax. A similar, though very much smaller, effect may have been produced by systematic error in the third Cambridge series, the Oxford and Paris series.

We may conclude that if these systematic discordances are real, and have produced any sensible effect, they have probably tended to make the parallax too small rather than too large. I am disposed myself to think that there is some reality in them, and that we cannot say that the results of a series of photographs made with different instruments are really homogeneous, though they have been reduced to the same system of carefully determined stars. If this is really the case it is hardly necessary to point out the futility of supposing that the most accurate results can be obtained by combining simultaneous observations at widely separated stations, especially if somewhat different systems of star places have been used.

When the values of the unknowns derived from this solution are substituted in the equations of condition given by the Minneapolis results we have the following results :—

	Mean Residual.		Average Residual.	
	in X.	in Y.	in X.	in Y.
	″	″	″	″
For the 10 single exposures	+0·212	+0·006	0·216	0·165
„ 11 multiple „	−0·247	+0·035	0·389	0·173

These show that the weight of one of the components of a multiple exposure is only about one-quarter of that of a single exposure. But, further, we have the curious result that the single exposures give uniformly large positive residuals, while those from the multiple exposures are strongly negative. I think that this anomaly adds weight to the reasons which compelled us

to decide that it is unsafe to use the Minneapolis measures until they have been examined further.

§ 20. *Search for Effect of Planet's Motion.*—It did not seem impossible *a priori* that the planet's motion might have some effect in displacing the mean centre of its image. I therefore took the Cambridge plates in two fashions, guiding upon planet and stars alternately. I have separated the equations belonging to the two series and analysed their X residuals. The differences are very small, and there is nothing to show that they are other than accidental. It may be concluded that the effect of the trail of the planet, if any, is uniform throughout, and that there is no systematic difference between plates following stars and following planet.

§ 21. *Search for Correction to Ephemeris depending on the Square of the Time.*—In order to discover whether the ephemeris requires correction by a term depending on the square of the time, the residuals were distributed into sixteen half-day groups. The following table gives the number of residuals in successive groups, and the means of the residuals in the X and Y equations :—

Group.	Number.	Mean Residuals. X Equations.	Y Equations.
		″	″
1	20	+ 0·006	+ 0·019
2	4	+ ·027	+ ·179
3	4	− ·062	− ·052
4	12	+ ·002	− ·076
5	32	+ ·016	− ·025
6	23	− ·025	+ ·052
7	37	− ·027	+ ·010
8	3	+ ·060	− ·082
9	1	− ·019	− ·115
10	8	+ ·004	− ·089
11	9	+ ·006	+ ·035
12	15	+ ·025	− ·056
13	22	+ ·041	+ ·012
14	2	+ ·051	− ·072
15	18	+ ·023	+ ·006
16	39	− ·029	·000

There is no evidence here of any sensible term depending on the square of the time. Only two or three of the above quantities are larger than twice what would be their probable value if they were fortuitous.

§ 22. *Search for an Inequality in the Motion of the Planet with the Period of its Light Variation.*—According to André (*A.N.* No. 3698) the variation of light of the planet has a period of $5^h\ 16^m$, during which there are two equal maxima and two slightly unequal minima. If we divide the nine days over which our observations extend into intervals of $5^h\ 16^m$, subdivide them again each into eight parts, and group together the residuals

from observations whose epochs fall into corresponding eighths, we have the following result :—

X *Equations. No. of Residuals and Mean for Each Group.*

I.		II.		III.		IV.	
31	+0″·030	26	+0″·023	55	−0″·025	24	−0″·005
V.		**VI.**		**VII.**		**VIII.**	
25	+0″·048	25	+0″·012	21	−0″·060	28	−0″·030

These figures show no periodicity in $5\frac{1}{4}$ hours. But if we add together groups I. and V., and so on, we have

I. V.		II. VI.		III. VII.		IV. VIII.	
56	+0″·038	51	+0″·017	76	−0″·034	52	−0″·020,

which seems to show an oscillation of the planet in the half-period of $2\frac{3}{4}$ hours.

Before discussing the probability that this is real let us examine the Y residuals in the same way.

Y *Equations.*

I. V.	II. VI.	III. VII.	IV. VIII.
−0″·006	−0″·002	+0″·001	+0″·026
−0·062	+0·007	+0·011	+0·001

Again, there is no evidence of a $5\frac{1}{4}$-hour period. Taking means of I. and V., &c., as before, we have

I. V.	II. VI.	III. VII.	IV. VIII.
−0″·034	+0″·002	+0″·004	+0″·013

which does not suggest any periodicity of the Y residual in the half-period.

If these means are accidental their probable values are about ±0″·013 in X and ±0″·012 in Y. It seems to me that we may consider the Y means accidental ; only one is larger than might be expected, and that depends upon an abnormally large result in group V., quite unsupported by group I.

The case of the X residuals is different. All four are greater than their probable values if accidental ; two are more than twice as great ; the two half-series give very much the same effect independently, and the balance of groups in opposite phases of the apparent periodicity is remarkably good.

The reality of the period is, moreover, confirmed by the discussion of residuals from an earlier solution, from somewhat different material, differently weighted, and starting from a different zero of time. These give an apparent periodicity of very nearly the same amplitude and phase.

It is easy to show that the apparent periodicity cannot be due to a chance arrangement of systematically discordant results from one or more observatories. The number of residuals contributed to the four groups is as follows :—

	I. V.	II. VI.	III. VII.	IV. VIII.
Algiers	11	6	10	4
Cambridge	24	20	29	31
Lick	5	8	6	9
Northfield	3	2	16	0
Oxford	8	10	8	4
Paris	5	5	7	4

I am inclined, therefore, to think that this result is real, and that the planet has a real oscillation in its X coordinates, with a period of about $2^h\ 38^m$ and a semi-amplitude of between $0''{\cdot}03$ and $0''{\cdot}04$.

Variations in light could be produced either by irregularity in form or of surface albedo; and either of these could also produce an oscillation in the apparent position of the planet. It is possible to invent arrangements of figure and albedo which could produce between them almost any desired variation of light or oscillation of centre, in either the period of rotation or half that period. But in general irregularities of figure alone will make the period of the principal oscillation of position twice that of the principal variation in light; while irregularities of albedo will make the two periods the same. In the absence of evidence as to the range and phase of the light variation, if any, at the date for which the apparent oscillation in position is found, it is premature to draw any conclusions at present. I am indebted to Mr. H. N. Russell for these remarks.

If the discordances in X given in § 19 are all really systematic they cannot, with the above grouping, produce an inequality with a total amplitude of $0''{\cdot}001$; neither can the inequality, if real, have produced the apparent systematic discordances, except perhaps in the case of Northfield, whose plates nearly all chance to fall in group III.

It remains to be seen whether the inequality in X can have produced a sensible effect on the value of the parallax. If we take the algebraic sums of the parallax factors in groups of equations of condition formed as before we have

I. V. +3 II. VI. +13 III. VII. +27 IV. VIII. −18.

The average residuals in these groups are respectively

Large +*ve* Small +*ve* Large −*ve* Small −*ve*.

The first has practically no effect; the second and fourth show a tendency to diminish the parallax, which is balanced by the tendency of the third to increase it. We may conclude that the periodic inequality in X has had very little if any effect upon the parallax derived from the present discussion. But it should be noted that, since nine periods are nearly equal to one day, it

might affect very seriously a set of plates taken at nearly the same time on several successive days.

§ 23. *Results of the Solution.*—Having shown that there is no sensible term depending on t^2 to be included in our equations of condition, that the assumed weights were fairly correct, that there are no very serious discordances between the results of different observatories, and that the periodic inequality in X which seems to exist cannot affect the parallax to any extent, we may take our first solution as definitive and set out the following results :—

P.E. of one equation of condition of weight unity :—

In X $\pm 0''\cdot100$. In Y $\pm 0''\cdot090$.

From the X equations :—

$\Delta_1\xi_0$, constant correction to the ephemeris

$$= [-2''\cdot245] + 0''\cdot0126 \pm 0''\cdot0071$$

$\Delta_2\xi_0$, correction varying with the time per day

$$= +0''\cdot0023 \pm 0''\cdot0030$$

$\Delta\pi$, correction to the adopted value of the solar parallax

$$= -0''\cdot0013 \pm 0''\cdot0052.$$

From the Y equations :—

$\Delta_1\eta_0$, constant correction to the ephemeris

$$= [+0''\cdot247] + 0''\cdot0385 \pm 0''\cdot0077$$

$\Delta_2\eta_0$, correction varying with the time per day

$$= -0''\cdot0522 \pm 0''\cdot0028$$

$\Delta\pi$, correction to the adopted value of the solar parallax

$$= -0''\cdot0136 \pm 0''\cdot0116.$$

Combining the two values of $\Delta\pi$ according to their weights we have

$$\begin{aligned} \text{From the X equations} \quad & 368\Delta\pi = -0''\cdot4784 \\ \text{,, Y ,,} \quad & 77\Delta\pi = -1\cdot0472 \\ & 445\Delta\pi = -1\cdot5256 \end{aligned}$$

whence $\Delta\pi = -0''\cdot0034 \pm 0''\cdot0047$.

And since the value of the solar parallax assumed in the computations was $8''\cdot80$, we have as the result of this discussion

$$\pi = 8''\cdot7966 \pm 0''\cdot0047.$$

Conclusion.

§ 24. This value of the solar parallax has no kind of claim to be called definitive. But it has just succeeded in fulfilling the

lesser of the two purposes with which this piece of work was undertaken—to derive from the photographs of *Eros* a value of the solar parallax with a probable error as small as that of the value found by Sir David Gill from his heliometer observations of minor planets, viz. $8''{\cdot}802 \pm 0''{\cdot}005$. The two values agree within the limits of their respective probable errors, and perhaps inspire the hope that a final discussion of all the photographs of *Eros* may set the directly observed value of the solar parallax upon so secure a basis that the indirect methods will be compelled to show cause for their discordance.

§ 25. Inasmuch as the principal object of this work was to discover what would happen when one tried to combine the results of a number of observatories into one solution, we may sum up very briefly the outcome of the experiment as follows:

The labour of forming the system of standard stars of considerable relative accuracy found its reward in the facility with which systematic errors were discovered. So soon as confidence in the accuracy of the system was established, the appearance of large residuals became the signal for a search after systematic error; and the search was not often in vain. If the error was found to increase rapidly from the centre, and the outer stars had to be rejected, there were generally enough standard stars near the centre to give a good solution. If the error proved to be guiding error, and the brighter stars were rejected, there remained enough stars of magnitude nearly equal to that of the planet. In fact the treatment of diverse material demands that the standard stars should be fainter, and more evenly distributed close to the planet's path, than are the *repère* stars. And one can hardly overestimate the advantages that arose from the perfect simplicity of the linear reductions in rectangular coordinates.

The finding of occasional guiding error is satisfactory, if only because it was quite certain *a priori* that it must from time to time occur. The absence of any evidence of hour-angle error is the more satisfactory, because that error might reasonably be feared. The discovery that the field of the Crossley reflector becomes useless immediately outside the limit of sensibly perfect definition leaves it still unexplained why the error should take the particular form that it does; while the quite unexpected large errors in the Algiers plates, taken with a refractor of standard pattern, cannot fail to inspire many stimulating doubts as to the absolute value of results obtained with one instrument alone. At the same time the elimination of the larger part of the systematic errors, which seems to have been achieved, assures us at once of the practicability of making a general solution, and of the difficulty of treating the results of any one observatory apart from the others.

The force of the latter conclusion is increased if one may accept the reality of the oscillation in the position of the planet of short period and semi-amplitude about $0''{\cdot}03$. This oscillation

might well be entangled with the parallax displacements in a quite considerable series of observations made at a single observatory ; it is completely separated from the parallax when a general solution is made ; and the search for it throughout the period of observation of the planet will make a beautiful test of the real delicacy of our results.

It seems that we may draw, from the experience gained in the work of the present paper, the conclusion that future work would be greatly facilitated by the adoption of a close system of standard stars. The formation of the system that I have used made a considerable part of the whole labour. But in future the task will be very much lightened, because it will be possible to make the star system depend upon the very extensive series of star places derived from the work of the four French observatories. If we have a standard system whose relative places are known with a probable error of a few hundredths of a second we can get as much accuracy as an individual plate is capable of giving by measuring the planet and eight or ten of these stars well distributed around it. With such a standard system we can discover systematic errors, provided that the residuals in the reduction of the stars are open to inspection ; but if any such error is found, it is of the greatest advantage to have at hand the original measured coordinates. It is doubtful whether those observatories whose aim is to contribute their results in the form most convenient for a general reduction of all the material could do better than publish simply the original measures. It will probably pay better to give the man who undertakes a general solution the means to carry out reduction *ab initio*, rather than to do any part of it before publication, for the discovery of some systematic error when the observations are combined with others will often necessitate a new reduction.

Finally, if we are able to admit that these conclusions are sound, we are led to the proposition that any observatory with photographs of *Eros* still unmeasured can make its contribution to the definitive determination of the solar parallax of greatest effect by agreeing to select its stars from a close standard system, and doing as speedily as possible the absolute minimum of work. It is the hope of the writer that he may be allowed to submit, in the near future, a selection of standard stars for consideration.

§ 26. I must acknowledge with gratitude the most efficient help which has been given me by Miss Julia Bell, of Girton College, and Miss Anne Malden, of Newnham College, and must thank the Committee administering the Government Grant Fund of the Royal Society for the means of securing this help, without which the work could not have been done at Cambridge.

Cambridge Observatory :
1904 *June* 21.

The Mass of Jupiter, and Corrections to the Elements of the Orbits of the Satellites from Heliometer Observations made at the Cape during the years 1901 *and* 1902. By Bryan Cookson, M.A.

This paper contains a short account of an investigation of the mass of *Jupiter*, and of corrections to the elements of the orbits of the four larger satellites (the full account giving all details will be published shortly in the *Annals* of the Cape Observatory). The investigation was undertaken at the suggestion of Sir David Gill, and it forms a continuation, and in some sense a completion, of his observations on the satellites made in 1891. The observations in the present case were made by the writer during a visit of two years to the Cape ; the instrument used was the 7-inch heliometer of the Cape Observatory ; the method of observation was the same as that adopted by Sir David Gill in 1891 ; and the method of reduction is practically the same as that used by Dr. de Sitter in his reduction of Gill's observations.

The positions of the satellites were observed relatively to one another by measuring with the heliometer the distance and position-angle of every pair of satellites, and these measured coordinates were compared with the same coordinates calculated from the data of Souillart's theory. In all heliometer observations of the satellites made previously to Sir David Gill's series the places of the satellites had been observed relatively to the limbs of *Jupiter* with results not very satisfactory ; thus Bessel found that whilst the probable error of an observation of a pair of stars was $\pm 0''{\cdot}10$, it reached just twice that amount in observations made with the same instrument on the satellites ; and there is no doubt that the larger part of this loss of accuracy is due to the difficulty in placing the point-like image of a satellite exactly on an ill-defined object like the limb of the planet. But by observing the satellites relatively to one another, instead of relatively to the limb, the accuracy of star measurements can be attained if not surpassed, for the discs of the satellites are ideal objects to observe. There is, however, a slight theoretical disadvantage which arises from the satellites being observed in pairs and not singly, namely, the fact that the corrections to the elements of the different orbits are considerably involved in one another in the equations of condition. When the normals are solved, this of course implies some loss of weight of the various unknowns, which is, however, much more than compensated for by the smaller probable error of an obser vation.

The Observations.

The observations that we are dealing with were made between 1901 June 24 and September 27, and between 1902 June 10 and October 4, that is, within two months of the oppositions of 1901 and 1902; only one or two opportunities of securing observations within those periods were missed. In the following table the total number of observations of each pair of satellites is shown; the figures under the letter "s" denote the number of observations in distance, those under "p" the number in position-angle.

Pair of Satellites	I.-II.		I.-III.		I.-IV.		II.-III.		II.-IV.		III.-IV.		Total	
	s	p	s	p	s	p	s	p	s	p	s	p	s	p
1901	29	30	27	28	28	28	27	28	30	30	27	27	168	171
1902	35	35	38	38	32	33	37	37	38	37	42	42	222	222
Total ...	64	65	65	66	60	61	64	65	68	67	69	69	390	393

Each observation gives rise to an equation of condition containing as a rule thirteen unknowns; we have therefore to deal with 783 such equations. But a few more words concerning the observations themselves must first be said.

It is of the greatest importance to have means of controlling the scale value and the equatorial adjustments of the heliometer.

This was effected by observing, together with the satellites, a pair of standard stars whose distance and position-angle were accurately known from carefully determined meridian places. There was one pair of standards for 1901 and another for 1902, so chosen that they were very near to *Jupiter* throughout the period of observation, that their position-angle only differed by a few degrees from the average position-angle of a pair of satellites, and that their distance was as great as possible. All measures of distance were then referred to the adopted distance of these standards, and the zero of position-angle depended upon their adopted position-angle. The standards for 1901 were 24 and 26 *Sagittarii*; their distance and position-angle were

$$6591''\cdot100 \text{ and } 84^\circ\ 20'\ 36'' \ (1901\cdot0)$$

those for 1902 were 21 and 23 (θ) *Capricorni*, their adopted distance and position-angle being

$$4486''\cdot930 \text{ and } 76^\circ\ 31'\ 9'' \ (1902\cdot0).$$

Any correction that it may be found desirable to apply to the adopted places of these stars can be allowed for at the end of the work; for a change in the adopted distance affects only the mass of *Jupiter*, and a change in position-angle is equivalent to a change in the position of the fundamental plane to which the orbits of the satellites are referred.

A complete observation of either standards or satellites

consisted of four independent pointings in distance and four in position-angle, and the pointings were so made that the mean epoch of the distance pointings very nearly coincided with the mean epoch of the position-angle pointings. The moment of each pointing was marked by the observer calling out "Stop!" An assistant noted and recorded the time to the nearest second by a chronometer, whose error was determined by comparison with the transit-circle clock before and after the night's work.

In making measures of distance the necessary readings were always taken to determine the "κ correction." This systematic correction arises from the fact that the observer does not superpose the two images, but he places them side by side, so that the imaginary line joining them is parallel to a side of a wire-square placed in the focus of the object-glass. A full explanation of this systematic error, and of the method of correcting the observed distance, has been given by Sir David Gill in the *Cape Annals*, vi. pp. 232–236, and more need not be said about it in this note, excepting that κ was directly determined for every observation, and the corresponding correction applied to all observed distances.

The corrections applied to instrumental distances were the following:—

1. Run of the micrometer used for reading the scales.
2. Refraction.
3. κ Correction.
4. Scale value to reduce to adopted distance of standards.

Those applied to instrumental position-angles were:—

(*a*) Refraction.
(*b*) Equatorial adjustments.
(*c*) Index correction.

The observations were not reduced to the centre of the Earth, because the correction on this account was more easily applied at the end of the computations.

The next step was to compare the observed coordinates thus corrected with the coordinates calculated from Souillart's theory.

The Tabular Places.

An ephemeris was constructed for the two periods of observation based on the expressions for the longitudes, radius vectors, and latitudes given by Souillart. These expressions were put into tabular form by Marth in *Monthly Notices*, vol. li., and his tables with one or two corrections were used in forming the ephemeris for the satellites. This gave the mean longitudes and the arguments of the inequalities at ten-day intervals, also the nodes and inclinations of the orbits. The adopted plane of reference was a plane which very nearly coincided with the plane of *Jupiter's* equator, the advantage of using which as reference plane was pointed out by Marth. An ephemeris was also formed to give the jovicentric coordinates of the Earth and the Sun referred to this plane. Referred to *Jupiter's* actual orbit, the reference

planes for the mean epochs of the two sets of observations are defined by

	Node (Ω)	Inclination
1901·61	315° 30′ 35″·2	3° 4′ 6″·6
1902·60	315° 31′ 29″·1	3° 4′ 7″·0

the node being reckoned from the Earth's mean equinox. The longitudes and nodes of the satellites are reckoned from the descending node (☊) of this plane.

As was pointed out by Professor Sampson in the *Observatory*, 1901 August, the values of the major axes of the satellites' orbits given by Marth are not quite correctly associated with the mass of *Jupiter* owing to his having neglected all the non-periodic perturbations of the axes, excepting that due to the compression of *Jupiter*. The correct values of log a'', a'' being the semi-major axis in seconds of arc at unit distance, are

2·764534 2·966232 3·169012 3·414255

which according to Marth would correspond, not to a mass of 1/1047·7, but to

1/1047·68 1/1047·56 1/1047·65 1/1047·45

Since in the equations of condition the corrections to the axes are eliminated by expressing them in terms of corrections to the mass of *Jupiter*, and to the constant of compression Jb^2, this is a point of importance.

The coordinates of the satellites were calculated from the expressions given by Marth (*Monthly Notices*, vol. li. p. 506) (with the necessary corrections), and from them were found the tabular distance and position-angle of a pair of satellites, which were then compared with the observed quantities. The differences between them were next expressed in terms of corrections to the constants of the theory.

The Unknowns.

Each of the six elements of a satellite's orbit was regarded as subject to correction. As there are four satellites this would give twenty-four unknowns. But as mentioned before the corrections to the axes were expressed in terms of two other unknowns, viz. the mass and the compression of *Jupiter*; moreover, following de Sitter, three other unknowns were introduced, namely, corrections to the coefficients of the large inequalities in the longitudes of the first three satellites. This leaves us with twenty-five unknowns, which it will be convenient to enumerate here. Let

$M =$ reciprocal of the mass of *Jupiter*.
$Jb^2 =$ constant of compression of *Jupiter*.
$\epsilon =$ longitude of epoch
$e =$ eccentricity
$\varpi =$ longitude of perijove
$\Omega =$ longitude of node
$\iota =$ inclination
} of a satellite.

Then the adopted unknowns were :—

$\mu = -\frac{100}{3}\frac{\Delta M}{M} = -\frac{\Delta M}{31{\cdot}43}$	denoted by μ	1
$\Delta(Jb^2)$	ΔJ	1
$\Delta(e \sin \varpi)$	Δh	4
$\Delta(e \cos \varpi)$	Δk	4
$\Delta\epsilon$	$\Delta\epsilon$	4
$\Delta(i \sin(-\Omega))$	Δp	4
$\Delta(i \cos(-\Omega))$	Δq	4
Δx	Δx	3
		25

where x is the coefficient of the great inequality in longitude.

After the coefficients of these quantities had been computed, and the complete equations of condition written out, normal equations were formed for each pair of satellites in the usual way. This was a heavy piece of work, and I wish to record my deep appreciation of the valuable assistance which was received in this, as in many other parts of the work, from Mr. V. A. Löwinger, of the Cape Observatory, who most kindly offered me his help in his spare time and enthusiastically gave it. For defraying a considerable part of the expense incurred in the formation of the normals for 1902 I am indebted to a grant from the Government Grant Committee of the Royal Society.

Solution of Normals.

After the normals had been formed, preliminary values of the unknowns were found by solving the equations for each pair of satellites, as it was of interest to see how far the values from the different pairs confirmed one another. Now it is clear that the inclinations and nodes of the orbits are to be determined chiefly from observations of position-angle; hence in the equations of condition derived from the observations of position-angle the coefficients of the unknowns depending on the nodes and inclinations, *i.e.* of Δp and Δq, are large, and those of the remaining unknowns are small, whilst in the distance equations the opposite is the case. In the preliminary solutions, therefore, the Δp's and Δq's were dropped out of the distance equations, and none except them were retained in the position-angle equations. We give below the values of the mass and of the corrections to the longitudes of the epoch as derived from the preliminary solutions of the distance equations and also from the general solution of all the normals added together to form one set of combined normals. The probable error of one observation of weight unity is determined in the usual way from the sum of the squares of the residuals.

		I.-II.	I.-III.	I.-IV.	II.-III.	II.-IV.	III.-IV.	General Solution.
1901								
Mass	...	1048·36	1047·39	1047·94	1047·45	1048·02	1047·65	1047·68
$\Delta\epsilon_1$	...	$+0^{\circ}\cdot168 \pm 0^{\circ}\cdot042$	$+0^{\circ}\cdot141 \pm 0^{\circ}\cdot014$	$+0^{\circ}\cdot126 \pm 0^{\circ}\cdot017$	...	...	...	$+0^{\circ}\cdot136 \pm 0^{\circ}\cdot010$
$\Delta\epsilon_2$	...	$+0^{\circ}\cdot021 \pm 0^{\circ}\cdot009$	...	...	$-0^{\circ}\cdot081 \pm 0^{\circ}\cdot027$	$+0^{\circ}\cdot028 \pm 0^{\circ}\cdot010$	...	$+0^{\circ}\cdot020 \pm 0^{\circ}\cdot006$
$\Delta\epsilon_3$	...	...	$-0^{\circ}\cdot062 \pm 0^{\circ}\cdot008$	...	$-0^{\circ}\cdot001 \pm 0^{\circ}\cdot010$	...	$-0^{\circ}\cdot041 \pm 0^{\circ}\cdot005$	$-0^{\circ}\cdot038 \pm 0^{\circ}\cdot005$
$\Delta\epsilon_4$	...	...	...	$-0^{\circ}\cdot041 \pm 0^{\circ}\cdot004$	...	$+0^{\circ}\cdot017 \pm 0^{\circ}\cdot003$	$-0^{\circ}\cdot035 \pm 0^{\circ}\cdot002$	$-0^{\circ}\cdot036 \pm 0^{\circ}\cdot002$
Prob Error	...	$\pm 0''\cdot071$	$\pm 0''\cdot079$	$0''\cdot104$	$0''\cdot100$	$0''\cdot089$	$0''\cdot060$	
1902								
Mass	...	1046·66	1046·89	1046·85	1047·33	1048·03	1047·92	1047·65
$\Delta\epsilon_1$	...	$+0^{\circ}\cdot035 \pm 0^{\circ}\cdot022$	$+0^{\circ}\cdot131 \pm 0^{\circ}\cdot010$	$+0^{\circ}\cdot153 \pm 0^{\circ}\cdot013$	...	...	...	$+0^{\circ}\cdot134 \pm 0^{\circ}\cdot008$
$\Delta\epsilon_2$	...	$-0^{\circ}\cdot017 \pm 0^{\circ}\cdot007$	...	...	$-0^{\circ}\cdot024 \pm 0^{\circ}\cdot014$	$-0^{\circ}\cdot025 \pm 0^{\circ}\cdot009$	...	$-0^{\circ}\cdot025 \pm 0^{\circ}\cdot005$
$\Delta\epsilon_3$	...	...	$-0^{\circ}\cdot025 \pm 0^{\circ}\cdot004$	...	$-0^{\circ}\cdot012 \pm 0^{\circ}\cdot005$	...	$-0^{\circ}\cdot028 \pm 0^{\circ}\cdot005$	$-0^{\circ}\cdot023 \pm 0^{\circ}\cdot003$
$\Delta\epsilon_4$	...	...	...	$-0^{\circ}\cdot030 \pm 0^{\circ}\cdot004$	...	$-0^{\circ}\cdot029 \pm 0^{\circ}\cdot004$	$-0^{\circ}\cdot039 \pm 0^{\circ}\cdot003$	$-0^{\circ}\cdot034 \pm 0^{\circ}\cdot002$
Prob. Error	...	$\pm 0''\cdot085$	$\pm 0''\cdot083$	$0''\cdot104$	$\pm 0''\cdot091$	$\pm 0''\cdot115$	$\pm 0''\cdot097$	

The different values of the mass are as accordant as can be expected; but as their weights were not determined, it is not possible to give their probable errors. The corrections to the longitudes of the epoch were the last two unknowns in each set of normals, and it was therefore an easy matter to find their weights. The values of the last-mentioned unknowns agree on the whole fairly well, but the values from II.–III. in 1901 are anomalous; so also is the value of $\Delta\epsilon_4$ from II.–IV. In 1902 the only outstanding one is $\Delta\epsilon_1$ from I.–II.

The next table gives the results of the preliminary solutions of the position-angle equations.

1901	I.–II.	I.–III.	I.–IV.	II.–III.	II.–IV.	III.–IV.	General Solution.
Δp_1	+0°·0340	+0°·0283	−0°·0206	...	...	...	+0°·0170
Δq_1	−0·0826	−0·0841	−0·0613	...	...	...	−0·0695
Δp_2	+0·1044	...	...	+0°·1181	+0°·1235	...	+0·1113
Δq_2	−0·1670	...	...	−0·1912	−0·1824	...	−0·1770
Δp_3	...	−0·0028	...	−0·0185	...	−0°·0103	−0·0148
Δq_3	...	−0·0424	...	−0·0465	...	−0·0284	−0·0360
Δp_4	...	...	+0·0461	...	+0·0516	+0·0447	+0·0456
Δq_4	...	...	−0·0431	...	−0·0321	−0·0334	−0·0359
1902							
Δp_1	+0·0114	+0·0272	−0·0295	...	...	...	+0·0137
Δq_1	−0·0844	−0·0908	−0·0534	...	...	...	−0·0755
Δp_2	+0·0968	...	...	+0·0820	+0·0996	...	+0·0922
Δq_2	−0·1797	...	...	−0·2012	−0·1783	...	−0·1860
Δp_3	...	−0·0042	...	−0·0173	...	−0·0036	−0·0072
Δq_3	...	−0·0428	...	−0·0277	...	−0·0297	−0·0334
Δp_4	...	...	−0·0754	...	+0·0631	+0·0632	+0·0658
Δq_4	...	...	−0·0089	...	−0·0073	−0·0061	−0·0070

It will be noticed at once that both in 1901 and 1902 the values of Δp_1 from I.–IV. differ widely from those given by the other two pairs; the values of Δp_3 and Δq_3 also vary more than was to be expected. With these exceptions the different determinations are in good agreement.

A complete solution was next made of all the normals, both in distance and position-angle, from every pair of satellites. As explained above the corrections to the inclinations and nodes were well separated from the other unknowns. This was especially the case in 1902, when the Earth lay almost in the planes of the orbits; in 1901 the Earth was more than 1° out of these planes, so that the separation of the unknowns was not so perfect as in 1902. In the case of the 1901 observations, therefore, a complete solution of the twenty-four combined normals was made.

The solution involved twenty-four and not twenty-five normals, because one of the unknowns, ΔJ, was transferred to the right-hand side of the equations, and all the other unknowns were expressed in terms of it, its value being left to be determined by other methods. The reason for doing this was that ΔJ could not be determined with sufficient weight from such a set of observations as the present, but was probably by no means a negligible quantity.

Let Δ denote the determinant formed by the coefficients in the normals, *i.e.* :—

$$\begin{vmatrix} [aa] & [ab] & [ac] & . & . \\ [ba] & [bb] & [bc] & . & . \\ [ca] & [cb] & [cc] & . & . \\ . & . & . & . & . \end{vmatrix}$$

and let Δ_{ik} be the first minor of $[ik]$; then the solution was made in the form,

$$x = \frac{\Delta_{aa}}{\Delta} X + \frac{\Delta_{ab}}{\Delta} Y + \frac{\Delta_{ac}}{\Delta} Z + \ldots$$

$$y = \frac{\Delta_{ba}}{\Delta} X + \frac{\Delta_{bb}}{\Delta} Y + \frac{\Delta_{bc}}{\Delta} Z + \ldots$$

$$. \quad . \quad . \quad . \quad . \quad . \quad .$$

where X is the absolute term in the normal for x and so on. Since $[ik] = [ki]$, it follows that the solution of the original normals is also a set of normals. The weight of an unknown k is Δ/Δ_{kk}, *e.g.* of z it is Δ/Δ_{cc}. The best account of the practical application of this form of solution is given by de Sitter in the appendix to his discussion of Sir D. Gill's observations. As Dr. de Sitter there remarks, this method deserves to be better known than it appears to be.

In this way values of the unknowns were found which exactly satisfied the combined normals.

In the case of the 1902 normals, owing to the better separation of the distance and position-angle unknowns, a good first approximation to the values of Δp, Δq was obtained from a solution of the position-angle normals, neglecting the other unknowns; similarly a good approximation to those other unknowns was found from the distance normals, neglecting the Δp's and Δq's. Then by an obvious process of approximation it was easy to find the values which would satisfy the combined normals from distance and position-angle observations together. These values of all the unknowns from the two sets of observations are given below. It should be remarked that owing to the approximate method of solution the weights of the 1902 unknowns are not rigorously correct, but can differ very little from the rigorous weights which would be those derived from the solution of the combined normals.

Values of Unknowns.

Unknown.	Value. 1901.	Probable Error.	Correction to Unknown for Error of J^{b_2}.	Weight.	Value. 1902.	Probable Error.	Correction to Unknown for Error of J^{b_2}.	Weight.
μ	+0·00069	±0·00295	− 0·1605 ΔJ	1163	+0·00169	±0·00202	− 0·1225 ΔJ	2795
Δh_1	+0°·0756	0°·0303	+ 0·974 ΔJ	11	−0°·0438	0°·0378	−10·081 ΔJ	8
Δk_1	−0·0843	0·0581	−16·940 ΔJ	3	−0·0087	0·0276	− 1·135 ΔJ	15
Δx_1	+0·0512	0·0503	+ 7·593 ΔJ	4	−0·0606	0·0378	− 9·015 ΔJ	8
Δh_2	−0·0188	0·0201	+ 2·533 ΔJ	25	−0·0412	0·0193	− 6·556 ΔJ	31
Δk_2	+0·0260	0·0335	+ 2·797 ΔJ	9	+0·0344	0·0169	+ 9·542 ΔJ	40
Δx_2	+0·0665	0·0318	−13·267 ΔJ	10	+0·1447	0·0205	+ 6·986 ΔJ	27
Δh_3	+0·0088	0·0147	+ 2·711 ΔJ	47	−0·0221	0·0125	− 3·370 ΔJ	73
Δk_3	−0·0789	0·0187	+ 8·900 ΔJ	29	−0·0165	0·0112	− 5·118 ΔJ	91
Δx_3	−0·0189	0·0214	+16·957 ΔJ	22	−0·0289	0·0133	− 4·069 ΔJ	65
Δh_4	+0·06942	0·00547	− 3·828 ΔJ	338	+0·08445	0·00372	− 0·544 ΔJ	824
Δk_4	+0·03301	0·00638	+ 1·742 ΔJ	249	+0·02261	0·00616	+ 3·507 ΔJ	301
$\Delta\epsilon_1$	+0·13587	0·00987	− 6·097 ΔJ	104	+0·13383	0·00754	+ 0·934 ΔJ	201
$\Delta\epsilon_2$	+0·01954	0·00630	+ 2·000 ΔJ	255	−0·02539	0·00460	− 2·304 ΔJ	541
$\Delta\epsilon_3$	−0·03803	0·00465	− 1·066 ΔJ	469	−0·02278	0·00292	− 0·705 ΔJ	1344
$\Delta\epsilon_4$	−0·03640	0·00196	− 0·911 ΔJ	2625	−0·03428	0·00173	+ 0·155 ΔJ	3811
Δp_1	+0·01696	0·00767	Negligible	172	+0·01366	0·00717	Negligible	223
Δq_1	−0·06946	0·00845	,,	142	−0·07548	0·00653	,,	269
Δp_2	+0·11134	0·00557	,,	326	+0·09218	0·00442	,,	587
Δq_2	−0·17697	0·00486	,,	428	−0·18604	0·00397	,,	728
Δp_3	−0·01484	0·00355	,,	802	−0·00715	0·00279	,,	1465
Δq_3	−0·03602	0·00327	,,	948	−0·03338	0·00232	,,	2120
Δp_4	+0·04563	0·00185	,,	2955	+0·06581	0·00147	,,	5236
Δq_4	−0·03592	±0·00176	,,	3253	−0·00699	±0·00144	,,	5493

Sum of squares of residuals = 6″·971 ... ... 10″·528

Number of equations of condition = 337 444

Probable error of one observation = ±0″·1006 ... ±0″·1068

The probable errors for the two sets of observations show that the above values of the unknowns satisfy the equations of condition fairly satisfactorily. In 1901 there are five residuals in the distance equations which exceed ±0″·3, viz. +0″·303, +0″·323, +0″·359, +0″·548 and −0″·551; in 1902 there are eight, viz. +0″·301, −0″·311, +0″·319, −0″·323, +0″·341, +0″·426, −0″·535, 0″·553. A re-examination of the reduction of the original observations and of the equations of condition may reduce some of these large residuals; this would materially reduce the two probable errors given above, but would affect the values of the unknowns but little.

A table of residuals grouped according to distance shows no dependence of the magnitude or sign of a residual on the distance observed, and it is concluded that the observations of distance are free from systematic error.

When we come, however, to inspect the residuals in the position-angle equations, we meet with a curious phenomenon, for which no plausible explanation can at present be found.

It was noticed that the residuals in the pairs I.–IV., II.–IV., and III.–IV. were markedly larger than in the other pairs, and that large residuals from the three pairs occurred on the same day ; moreover the same change in the y coordinate of IV. would cause all these residuals practically to vanish. Thus the y, i.e. the latitude of IV. seemed to be in error. The mean of the residuals from I.–IV., II.–IV., and III.–IV. was therefore plotted for each day by observation, and it then appeared that a period of eight days would fit the graph of residuals. The mean of the residuals for IV. was therefore plotted with twice the mean longitude of IV. as abscissæ—this corresponds to a period of 8·23 days. The result was that for both years of observation the residuals obviously lay about a sine curve, that is, the mean residual of I.–IV., II.–IV., and III.–IV. could be approximately represented by the expressions

$$\text{Residual} \times \operatorname{cosec}(p-\text{P}) = 0''\cdot050 + 0''\cdot150 \sin(2l_4 + 10^\circ) \text{ in } 1901$$
$$= 0''\cdot100 + 0''\cdot150 \sin(2l_4 - 100^\circ) \text{ in } 1902$$

Here p is the position-angle of the pair of satellites, and P is the position-angle of *Jupiter's* axis. If we apply these empirical corrections to the mean residual for IV., we get a much better representation of the observations ; thus in 1901 the sum of the squares of the mean residuals is $0''\cdot858$, whilst of the mean residuals corrected as above it is $0''\cdot628$—a reduction of 27 per cent. In 1902 it was desired to see the effect of the periodic term alone ; therefore $0''\cdot100$ was subtracted from each mean residual $\times$ cosec $(p-\text{P})$ and the sum of the squares found : it amounted to $0''\cdot587$. But on taking out the periodic as well as the constant term, the sum of the squares was reduced to $0''\cdot356$—a reduction of 39 per cent.

It seems clear, then, that the observations are much better represented by an empirical term in the latitude of IV., which is

$$\text{in } 1901 \qquad \Delta\lambda_4 = 17'' + 50'' \sin(2l_4 + 10^\circ)$$
$$\text{in } 1902 \qquad \Delta\lambda_4 = 32'' + 48'' \sin(2l_4 - 100^\circ)$$

To make the phases for the two years the same, it would be necessary to change the period from $8^d\cdot23$ to $8^d\cdot08$, which would have practically no effect on the representation of the observations by the curve ; thus we may say that the same periodic term with a period of $8^d\cdot08$ represents the observations of both years.

This inequality is most probably the cause of the abnormal values of Δp_1 and Δq_1 from I.–IV., which were shown in the preliminary solutions for both years.

No satisfactory explanation of this has yet been found and therefore the values of the unknowns have not been corrected on account of it ; but of the reality of its existence there seems to be no doubt. It should be added that the observations of Sir D. Gill, discussed by Dr. de Sitter, do not show a similar systematic run, though there are one or two days when the same change in the latitude of IV. would reduce large residuals.

Results.

As the corrections to all the elements have been expressed in terms of the unknown ΔJ, we first proceed to determine its probable value. As is well known, one effect of the planet's equatorial protuberance is to cause the perijove of a satellite and its node on the planet's equator to revolve at equal rates, the former with a direct motion, the latter with a retrograde. The eccentricities of the orbits of the three inner satellites being but slight, we can only make use of the motion of the node for the determination of the compression constant Jb^2. The fourth is not suitable, because at its distance from *Jupiter* the compression causes only a very slow motion of perijove or node. The second satellite has a considerable inclination to *Jupiter's* equator, and the motion of its node is almost entirely due to the compression of the planet, only a very small part of it being due to the action of the other satellites. We shall therefore attempt to find Jb^2 from the motion of the node of II., using the present and earlier observations.

The quantity to be determined is the motion of the node of the orbit of II. on its "invariable" plane—*i.e.* the quantity b_1 of Laplace or Souillart. In the latter's notation the invariable plane referred to the fixed plane of reference—i.e. *Jupiter's* orbit of 1850, has its node $=b_4t+\gamma_4$ and inclination $=N_4'$.

Observation has given the following for the orbit of II. referred to its invariable plane :—

	Inclination × sine (Node).	Inclination × cos (Node).
1879·00	+0°·0184	−0°·4432
1891·75	+0·2099	+0·4808
1901·61	−0·5144	+0·1035
1902·60	−0·4957	+0·0067

For the epoch 1879·00 the above quantities, generally denoted by p and q, have been deduced from Schur's paper *Bestimmung der Masse des Planeten Jupiter* : for 1891·75 the values given by de Sitter in his discussion of Gill's observations have been used. The other two epochs are those of the writer's observations. It

should be said that the nodes are reckoned from the Earth's moving equinox.

Now, in Souillart's notation the theoretical expressions of the above quantities are

$$p = N_1' \sin(b_1 t + \gamma_1) + 102''{\cdot}3 \sin(b_2 t + \gamma_2) + 15''{\cdot}7 \sin(b_3 t + \gamma_3) + 2''{\cdot}8 \sin(2L - b_4 t - \gamma_4)$$

q = same with cosines.

Whence N_1' and $b_1 t + \gamma_1$, on the assumption that the other coefficients and angles are correct. In this way we find

	N_1'	$b_1 t + \gamma_1$
1879·00	0°·4733	179°·38 ± 2°·40
1891·75	0·4956	25·45 ± 0·37
1901·61	0·5010	278·65 ± 0·65
1902·60	0·4793	267·48 ± 0·52

The probable errors given after $b_1 t + \gamma_1$ are those of the node of the orbit on the planet's equator, not of $b_1 t + \gamma_1$, and the relative weights of the different determinations may be taken as inversely proportional to the squares of these probable errors. Comparing with Souillart's values we get the following equations of condition from which to find corrections to his values of b_1 and γ_1 (1891·75) :—

$-4657\Delta b_1 + \Delta\gamma_1 = 13°{\cdot}21$	*wt* 0·16	residual	$-5°{\cdot}35$
$\Delta\gamma_1 = 12·84$	7·29		−1·78
$3605\Delta b_1 + \Delta\gamma_1 = 24·78$	2·25		+1·01
$3963\Delta b_1 + \Delta\gamma_1 = 25·44$	3·61		+0·52

whence

$$\Delta b_1 = 0°{\cdot}00294 \pm 0°{\cdot}00037 \text{ and } b_1 = -0°{\cdot}03003 \pm 0°{\cdot}00037$$
$$\Delta\gamma_1 = +13·51 \pm 0·96$$

It may be doubtful whether the first equation should be included or not. No details were given by Schur as to his method of observing position-angles ; he found, moreover, that his observations were better represented by assuming the existence of a large empirical term depending on whether the satellite followed or preceded the planet. If we assign weight zero to the first equation we get

$$\Delta b_1 = 0°{\cdot}00322 \pm 0°{\cdot}00012 \text{ and } b_1 = -0°{\cdot}02975 \pm 0°{\cdot}00012$$
$$\Delta\gamma_1 = +12·85 \pm 0·30$$

These probable errors are calculated in the usual way from the sum of the squares of the residuals ; but a truer estimate of the probable error of b_1 can be found from the consideration

that the probable error of the difference between the mean $b_1 t + \gamma_1$ of 1901 and 1902 and that of 1891 is $\pm 1/203$ of that difference ; whence the probable error of b_1 is $\pm 0^\circ\cdot 00015$.

From Souillart's tables we have

$$0\cdot 8230 \frac{\Delta J}{J} + 0\cdot 1297 \frac{\Delta m''}{m''} = \frac{\Delta \lfloor 1 \rfloor}{\lfloor 1 \rfloor} = -\frac{\Delta b_1}{\lfloor 1 \rfloor}$$

m'' being the mass of III. The adopted value of J is 0·02472, the planet's diameter being $36''\cdot 97$: the corresponding value for $39''\cdot 00$ is 0·02221.

Hence with

$$J = 0\cdot 02221 \left(\frac{39''\cdot 00}{d}\right)^2$$

the foregoing equation may be written

$$\Delta J = -0\cdot 8212 \Delta b_1 - 0\cdot 00351 \Delta m''/m''$$

We have, therefore, neglecting $\Delta m''$ for the moment

$\Delta J = -0\cdot 00241$ or $Jb^2 = 0\cdot 01980 \pm 0\cdot 00030$, including Schur's obs.
$\Delta J = -0\cdot 00273$ or $Jb^2 = 0\cdot 01948 \pm 0\cdot 00012$, excluding ,,

From the motion of the orbit of the fifth satellite Cohn (*Astr. Nach.* 3403) found

$$J = 0\cdot 02095 \times \left(\frac{39''\cdot 00}{d}\right)^2$$

corresponding to his observed annual motion of the perijove, viz. 912°. The latest observations of Barnard, however, indicate that this annual motion is too large, *i.e.* that Cohn's J needs a negative correction (*Astronomical Journal*, No. 544).* For a first approximation we shall take

$$Jb^2 = 0\cdot 01955 \pm 0\cdot 00015$$

as the value from the motion of the node of II. ; this corresponds to an annual motion of 851° of the perijove of V.

From Bessel's, Schur's, Gill's, and the writer's observations, embracing altogether a period of sixty-six years, it was found from the motion of the perijove of IV. that

$$\Delta m''/m'' = -0\cdot 063 \pm 0\cdot 028$$

with the above value of Jb^2.

If this value be nearer the truth than $\Delta m'' = 0$, we have

$$Jb^2 = 0\cdot 01977$$

* In the *Astronomical Journal*, 1904 May 20, Miss Dobbin finds 887° for the motion of the perijove from Professor Barnard's observations in 1898–1903.

It is interesting to see what confirmation of this value can be found from the node of III. Treating it in the same way, we find from Schur's re-reduction of Bessel's observations that

$$\begin{aligned} \text{in } 1836 \quad & b_2 t + \gamma_2 = 120{\cdot}3 \\ \text{in } 1902 \quad & = 321{\cdot}1 \end{aligned}$$

giving a motion of 159·2 in sixty-six years against Souillart's 168·3—*i.e.* a reduction of 1/18·49. The theoretical expression for Δb_2 is

$$-\frac{\Delta b_2}{[2]} = 0{\cdot}752\frac{\Delta J}{J} + 0{\cdot}126\frac{\Delta m'}{m'} + 0{\cdot}088\frac{\Delta m''}{m''} + 0{\cdot}019\frac{\Delta m}{m}$$

neglecting the correction to the Sun's mass. Now, as will be seen further on, the mass of II. is certainly not much in error; hence we may safely neglect $\Delta m'$. And the other two terms may be neglected on account of the smallness of their coefficients. With sufficient accuracy we have therefore

$$0{\cdot}752\Delta J/J = -1/18{\cdot}49$$

whence

$$Jb^2 = 0{\cdot}02062 \pm 0{\cdot}00051$$

The probable error is only roughly determined from the fact that the above difference of 159° has a probable error amounting to about 1/40 of itself.

These two determinations of Jb^2 agree nearly within the limits of their probable errors, and the conclusion is that the value

$$J = 0{\cdot}01987\left(\frac{39''{\cdot}09}{d}\right)^2$$

is the value which best satisfies the existing heliometer observations of the satellites. This would give 863° for the annual motion of the perijove of V., and it will be of extreme interest to see how nearly a comparison of the latest observations of V. with the earliest reproduces this value. It must be remembered that the expression for the motion of the perijove is

$$\frac{\partial P}{\partial t} = n\left(\frac{Jb^2}{a^2} + \frac{Kb^4}{a^4} + \ldots\right)$$

and that, as de Sitter pointed out, the second term in the case of V. amounts to 1/50 of the first, and therefore cannot be neglected. The inclusion of this term raises the value of the motion of the perijove to 880°, which does not differ much from the value Miss Dobbin finds from observation, viz. 887°. Part of the difference between the value of Jb^2 as derived from II. and that from V. is thus due to the term containing the constant

K. The importance of the comparison of the two values is obvious.

Mass of Jupiter.—The reciprocal of the mass is

1901 $1047{\cdot}678 \pm 0{\cdot}093 + 5{\cdot}045\Delta J$

1902 $1047{\cdot}647 \pm 0{\cdot}059 + 3{\cdot}850\Delta J$

This requires a small correction, viz. $+0{\cdot}024$ to reduce the observations to the centre of the Earth. Applying this and introducing the value of ΔJ, we have

1901 $1047{\cdot}690 \pm 0{\cdot}093 - 0{\cdot}4769\delta\sigma_1$

1902 $1047{\cdot}662 \pm 0{\cdot}059 - 0{\cdot}7005\delta\sigma_2$

where $\delta\sigma$ is the correction in seconds of arc to the adopted distance of the standard stars.

An alteration in their adopted distance affects the determination of the mass and the mass only. It is therefore of great importance to determine the meridian places of the standards with the utmost accuracy. This is being done by several observatories at Sir David Gill's request, but the results are not yet to hand. It is not likely, however, that the adopted places require much alteration, as they have been derived from a discussion of all existing material. The preliminary value of the mass from the writer's observations is thus

$$1047{\cdot}67 \pm 0{\cdot}06$$

a value which differs considerably from Newcomb's, viz.

$$1047{\cdot}35.$$

Coefficients of Great Inequalities.—The substitution of the value of ΔJ gives for the corrections to these :—

	1901.	1902.	Mean.
Δx_1	$+0^{\circ}{\cdot}0327 \pm 0^{\circ}{\cdot}0503$	$-0^{\circ}{\cdot}0386 \pm 0^{\circ}{\cdot}0378$	$-0^{\circ}{\cdot}0127 \pm 0^{\circ}{\cdot}0303$
Δx_2	$+0{\cdot}0989 \pm 0{\cdot}0318$	$+0{\cdot}1276 \pm 0{\cdot}0205$	$+0{\cdot}1162 \pm 0{\cdot}0172$
Δx_3	$-0{\cdot}0600 \pm 0{\cdot}0214$	$-0{\cdot}0190 \pm 0{\cdot}0133$	$-0{\cdot}0304 \pm 0{\cdot}0114$

It appears, then, that x_1 and x_3 are practically in need of no correction ; as these two coefficients are functions of the mass of II., it follows that the adopted mass of II. is correct. But the correction to x_2 seems to be real, and as the theoretical expression for Δx_2 is

$$\Delta x_2 = 1{\cdot}09\Delta m_3 + 0{\cdot}62\Delta m_1$$

we have

$$\Delta m_1 = 0{\cdot}187 \pm 0{\cdot}028 - 1{\cdot}75\Delta m_3$$

This correction to x_2 is very large, and is not confirmed by the value Dr. de Sitter found. He gives

$$\Delta x_2 = -0^{\circ}{\cdot}0178 \pm 0^{\circ}{\cdot}0156$$

But on the whole the evidence goes to show that x_2 is in need of a position correction of about $0^\circ{\cdot}05$. This would bring the adopted value of Souillart, *i.e.* 3693″, into near agreement with Damoiseau's value, *i.e.* 3867″. It would also appear that Adams' value of the mass of I. is nearer the truth than Damoiseau's.

Jupiter's Equator.—Owing to the great equatorial protuberance of the planet the determination of what may be called its dynamical equator is of importance. To determine its position we have referred the orbits of I., III., and IV. as given by observation, to Souillart's fixed plane, *i.e.* the planet's orbit of 1850. Then in Souillart's notation we have

$$N_4 \sin(b_4 t + \gamma_4) = \text{observed } p - N \sin(bt + \gamma) - N_1 \sin(b_1 t + \gamma_1) - \&c.$$

$$N_4 \cos(b_4 t + \gamma_4) = \text{observed } q - \text{same with cosines.}$$

Hence assuming his values of the coefficients and angles, and using his values of σ_4'' σ_4''', which, be it remembered, depend on the adopted masses, we get the following values of N_4 and of $b_4 t + \gamma_4$:—

	N_4.			$b_4 t + \gamma_4$.			
	I.	III.	IV.	I.	III.	IV.	Souillart.
1891·75	3°·0928	3°·1486	3°·0637	316°·07	315°·46	316°·68	315°·32
1901·61	3·1355	3·0997	3·0888	315·65	315·22	316·47	315·45
1902·60	3·1417	3·0974	3·0573	315·59	315·37	316·91	315·46

The values for 1891·75 have been deduced from de Sitter's paper. If weights are assigned inversely proportional to the squares of the probable errors, we have

From	I. $N_4 = 3°{\cdot}1190$	at 6·8	Weighted mean ... $3°{\cdot}0806 \pm 0°{\cdot}0094$
,,	III. $= 3{\cdot}1207$	,, 45·2	
,,	IV. $= 3{\cdot}0666$	,, 147·8	Souillart ... $3°{\cdot}0658$

It follows that the observations of III. give for the inclination of *Jupiter's* equator to his orbit

$$\omega = 3^\circ\ 7'\ 22''$$

whereas the fourth satellite gives

$$\omega = 3^\circ\ 4'\ 8''$$

the value adopted by Souillart from Damoiseau being

$$\omega = 3^\circ\ 4'\ 5''$$

It is a significant fact that the value from IV. is $3'\ 14''$ less than that from III., Damoiseau having also found a difference of $2'\ 47''$ in the same sense. This difference is probably due to errors in the adopted masses and in Jb^2.

It may be mentioned that if the value of Jb^2 found above is used instead of the value adopted by Souillart, the difference is diminished ; but it was not thought worth while resolving the necessary equations (see Laplace, *Méc. Cél.* Pt. II. livre viii. § 23) in our present state of ignorance as to the masses, especially those of I. and IV.

Assuming b_4 known, we get for the corrections to Souillart's $b_4t+\gamma_4$ at the epoch 1891·75 the following values :—

I.	$+0^\circ\ 27'$
III.	$-0\ \ 4$
IV.	$+1\ \ 17$

These determinations of the equator rest on the observations of position-angle, and we have seen that in 1901 and 1902 the observations of IV. are affected with a systematic residual. The periodic part of this error, having a period of only eight days, can have but little effect on the values of the unknowns, though it increases their probable errors : the constant part, however, does slightly affect this determination of the equator from IV.

Longitudes of Epoch.—As is well known, the mean longitudes satisfy the relation

$$\epsilon_1-3\epsilon_2+2\epsilon_3=180^\circ.$$

The corrections which we have found will therefore satisfy

$$\Delta\epsilon_1-3\Delta\epsilon_2+2\Delta\epsilon_3=0$$

provided no inequality is included in any of these corrections. Now it was proved by Laplace on theoretical grounds that there is an oscillation or libration about the state represented by the equation first written, *i.e.* that

$$\epsilon_1-3\epsilon_2+2\epsilon_3=\text{not } 180^\circ, \text{ but } 180^\circ-\mathrm{D}\sin(bt+\mathrm{E})$$

where D and E are two constants of integration to be determined by observation. This libration was searched for by Delambre but without success, and it was concluded that the coefficient D was too small to be shown by observation.

It appears, however, from the present series of observations, and also from that of 1891, that this is not the case.

If we put $\theta=\epsilon_1-3\epsilon_2+2\epsilon_3$, observation gives

1901·61	$\theta=$	$180^\circ\cdot0012\pm0^\circ\cdot0230-14\cdot229\Delta J$
1902·60		$180\cdot1644\pm0\cdot0170+\ 6\cdot436\Delta J$

whence

1901·61	$\theta=$	$180^\circ\cdot035\pm0^\circ\cdot023$
1902·62		$180\cdot148\pm0\cdot017$

If we adopt Laplace's theoretical period, viz. 6·215 years, and reckon the time from 1901·61, we find that these values are represented by

$$\theta = 180° - 0°·156 \sin (bt + 193°)$$

which gives for the epoch 1891·75

$$\theta = 180°·049$$

Dr. de Sitter found from the observations at this epoch a value practically in perfect agreement, viz.

$$\theta = 180°·053 \pm 0°·014$$

The period 6·215 years depends on the adopted values of the masses. If we take Adams' values, the period is reduced to 5·36 years, which would give

$$\theta = 180° - 0°·151 \sin (bt + 193°)$$
$$\theta \text{ at epoch } 1891·75 = 180°·141$$

This does not agree with the observed value, and though it may require a small correction on account of ΔJ, yet such correction cannot be sufficient to bring the two into agreement.

In view of the uncertainty as to the true values of the masses, and consequently of the period of the libration, we cannot determine its coefficient and phase by any method involving an accurate knowledge of its period.

The libration is distributed amongst the longitudes as follows, according as Damoiseau's or Adams' masses are adopted :—

I.	δl	$= -0°·274\,D \sin (bt+E)$ or	$-0°·178\,D \sin (bt+E)$
II.	$\delta l'$	$= +0°·231\,D \sin (bt+E)$	$+0°·262\,D \sin (bt+E)$
III.	$\delta l''$	$= -0°·016\,D \sin (bt+E)$	$-0°·019\,D \sin (bt+E)$

The substitution of the above values of D gives for the coefficients of sin $(bt+E)$:—

$-0°·043$	$+0°·036$	$-0°·002$	Damoiseau.
$-0°·026$	$+0°·040$	$-0°·003$	Adams.

The corrections to the mean longitudes as found from observation are :—

	1891.	1901.	1902.
$\Delta_1\epsilon$	$+0°·102 \pm 0°·006$	$+0°·151 \pm 0°·010$	$+0°·132 \pm 0°·007$
$\Delta\epsilon_2$	$-0°·001 \pm 0°·004$	$+0°·015 \pm 0°·006$	$-0°·020 \pm 0°·004$
$\Delta\epsilon_3$	$-0°·026 \pm 0°·002$	$-0°·035 \pm 0°·005$	$-0°·021 \pm 0°·003$

[The values for 1891 are taken from Dr. de Sitter's paper.] These corrections when cleared of libration become :—

		1891.	1901.	1902.
Damoiseau's Masses	$(\Delta\epsilon_1)$	$+0^\circ\cdot088$	$+0^\circ\cdot140$	$+0^\circ\cdot091$
	$(\Delta\epsilon_2)$	$+0\cdot010$	$+0\cdot024$	$+0\cdot015$
	$(\Delta\epsilon_3)$	$-0\cdot027$	$-0\cdot035$	$-0\cdot020$
Adams' Masses	$(\Delta\epsilon_1)$	$+0\cdot126$	$+0\cdot145$	$+0\cdot108$
	$(\Delta\epsilon_2)$	$-0\cdot038$	$+0\cdot024$	$+0\cdot018$
	$(\Delta\epsilon_3)$	$-0\cdot023$	$-0\cdot036$	$-0\cdot024$

From this it would appear that the observations are fairly well represented by a libration into a coefficient of $0^\circ\cdot156$ and a period calculated with Damoiseau's masses, though the difference between the values of $(\Delta\epsilon_1)$ amounts to four times its probable error. If, on the other hand, Adams' masses are the more correct, as appears from other evidence, then the value of $(\Delta\epsilon_2)$ for 1891 requires explanation. In this connection it may be mentioned that in the longitude of II. there are some large inequalities with coefficients amounting to $0^\circ\cdot04$, and with long periods (500 days), and that any possible correction to these would be included in the observed value of $\Delta\epsilon_2$.

The coefficient of the libration comes out surprisingly large, so large as to raise doubts about its being in reality the libration. However, the old eclipse observations which Delambre discussed might quite possibly fail to show it, for it could only accelerate or retard an eclipse by 30^s ; a quantity well within the limits of the errors of observation ; and unless a good number of eclipses of all three satellites were observed within a few months of one another—a practical impossibility in the case of III.—the observations would have to be combined by assuming a period. If the assumed period were not correct, it would be impossible to detect the libration.

The two quantities which at the present time it is most desirable to determine from observation are the motion of the perijove of IV. and the two eccentricities of III. Until these have been determined the masses and all depending on them will remain in their present state of uncertainty.

It may be worth while to give the four best determinations of the masses to show how vague our present knowledge of them is :

	Laplace.	Damoiseau.	Souillart.	Adams.
Mass of I.	0·1733	0·1688	0·3773	0·2831
II.	0·2324	0·2323	0·2453	0·2324
III.	0·8850	0·8844	0·8218	0·8125
IV.	0·4266	0·4248	0·2312	0·2149

The values adopted by Souillart in his theory of the satellites are Damoiseau's. The above values found by him as well as those of Adams are those which arise from the introduction of

Damoiseau's revised value, viz. $65^s{\cdot}073$, of the second eccentricity of III. instead of his earlier value, viz. $116^s{\cdot}73$. From this table it appears that the mass of II. is the only one that is known with any degree of certainty: the mass of III. is doubtful to the extent of not more than 10 per cent., but the masses of I. and IV. are at present so uncertain that they may almost be looked upon as unknown. We may, however, confidently expect that much of this uncertainty will be removed by the publication of Professor Sampson's discussion of the Harvard eclipses, which is therefore anxiously awaited.

In conclusion I should like to say how grateful I am to Sir David Gill for having given me an opportunity of carrying out this piece of work: for this and many other kindnesses, too numerous to mention, I wish to offer him my most sincere thanks. It is a pleasure also to acknowledge the unfailing courtesy and kindness I received from Mr. S. S. Hough and all the staff of the Cape Observatory during the two years I spent with them.

Note on the Distribution of Sun-spots in Heliographic Latitude, 1874 to 1902. By E. Walter Maunder.

Rather more than a year ago the Astronomer Royal communicated to this Society a paper which he had desired me to prepare on the "Mean Daily Areas of Sun-spots for each Degree of Solar Latitude for each Year from 1874 to 1902 as Measured on Photographs at the Royal Observatory, Greenwich." The paper was communicated at the Meeting of 1903 May 8, and printed in vol. lxiii. No. 8 of the *Monthly Notices*. It summarised in both tabular and graphical forms the results of thirty years' work in the measurement and reduction of about 9000 photographs of the Sun, and of the latitudes and areas of about 5000 separate groups of spots, involving upwards of a quarter of a million measures made in duplicate upon those photographs.

It appeared to me, when preparing the above paper, that the results given in it could be exhibited in somewhat different aspects, and I therefore prepared other diagrams at the same time, which were not communicated to this Society, but were exhibited at the Official Visitation of the Royal Observatory on 1903 June 6. These diagrams, altered only as to scale, so as to render them suitable for reproduction in the *Monthly Notices*, I would now submit to the consideration of the Royal Astronomical Society.

At the same time I would wish to supplement them by other diagrams which the appearance of a paper by Dr. W. J. S. Lockyer in the Appendix to the last number of the *Monthly Notices* has seemed to me to render necessary. A considerable

portion of Dr. Lockyer's paper deals exclusively with the numbers and diagrams presented in the Greenwich paper referred to above, and as, in my opinion, his conclusions are entirely unsupported by the facts of the case, it appears important to show what the Greenwich record of the Sun's surface does really teach.

The diagrams here presented are eight in number ; the first and the fourth, fifth, and sixth were drawn up, as already explained, more than a year ago ; the seventh and eighth were specially prepared with reference to Dr. Lockyer's paper on "Sun-spot Variation in Latitude, 1861–1902." The first deals with only the areas of Sun-spots ; the second to the sixth deal with both their areas and latitudes ; the last two with their latitudes alone.

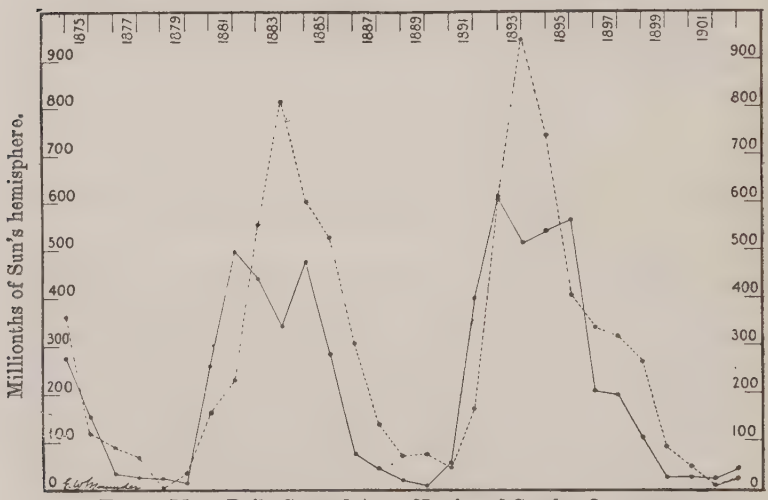


FIG. 1. Mean Daily Spotted Area, North and South, 1874–1902.
Northern Hemisphere —— Southern .....

The chief purpose of the first six diagrams is to call attention to the individuality shown by the two hemispheres of the Sun, north and south, the variations in the extent and in the mean latitude of the spotted area of the northern hemisphere being compared with the variations in the corresponding elements in the southern hemisphere. Incidentally these six diagrams serve to illustrate "Spoerer's Law of Spot-zones" ; but the seventh and eighth are especially designed to bear upon it, to illustrate the fidelity with which it represents the facts of the case in general, and to suggest some additions which can now be made to it.

Fig. 1 shows the mean daily spotted area of the two hemispheres, north and south of the Sun, taken out year by year for each year from 1874 to 1902 inclusive. The points for the

areas of the northern hemisphere are joined by a continuous line, those of the southern hemisphere by a broken line. The chief facts to be gathered from this diagram are :

(*a*) The two hemispheres do not, on the whole period, differ very greatly in total spotted area ; but the southern is the more prolific of the two, the northern claiming $43\frac{1}{2}$ per cent. of all spots, reckoning by area ; the southern $56\frac{1}{2}$ per cent.

(*b*) The critical points in the progress of the solar cycle are marked earlier, in both the two cycles shown, by the northern hemisphere than by the southern ; and this on both the ascending and descending parts of the curve.

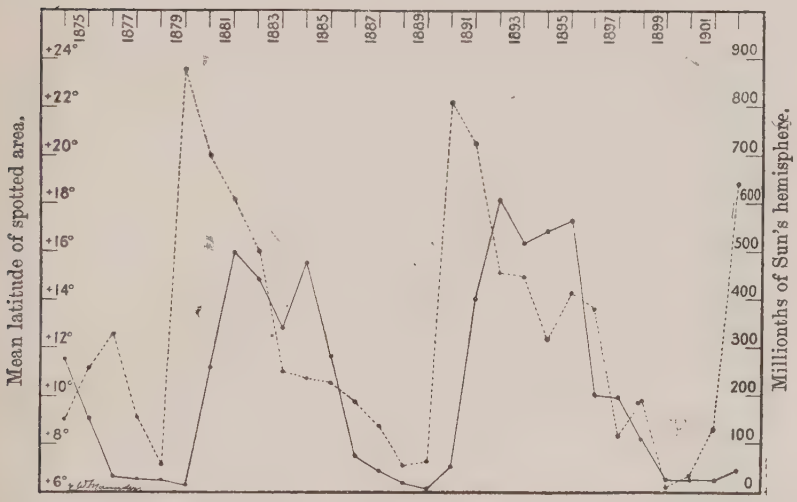


FIG. 2. Northern Hemisphere. Mean Latitude and mean Spotted Area, 1874–1902.

Latitude ..... Area ——

(*c*) The northern hemisphere shows a double maximum in both cycles, the first falling about three years before the second. The general form of the northern spot-curve, therefore, shows a long-continued but not very pronounced maximum.

(*d*) The southern hemisphere, on the contrary, shows but one very sharply marked maximum, synchronous with the slight depression between the two maxima of the northern curve.

Fig. 2 shows the mean daily spotted area for the northern hemisphere of the Sun for each year from 1874 to 1902, precisely as in fig. 1. But the mean heliographic latitude, year by year, of the spots of the northern hemisphere has also been indicated ;

the several points of the area curve being joined by a continuous line, those for the latitude curve by a broken line. The means for the latitudes are weighted means; the latitude for each separate spot-centre having been weighted in exact proportion to the area of the spot-group.

Fig. 3 shows for the southern hemisphere the same particulars as those given by fig. 2 for the northern. The following seem to be the principal relations brought out by the two diagrams:

(*e*) The rise in latitude is very abrupt in both hemispheres and takes place just as the minimum in spotted area comes to an end. The highest mean latitude is attained about three and a half years before the mean maximum* in area.

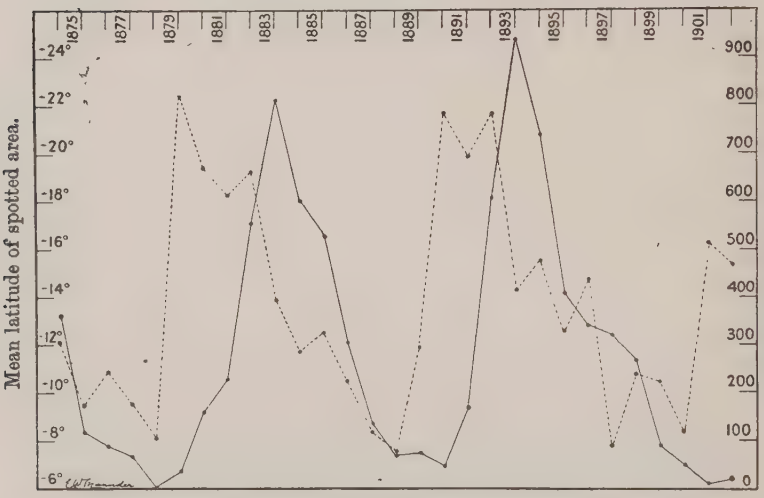


FIG. 3. Southern Hemisphere. Mean Latitude and mean Spotted Area, 1874–1902.
Latitude..... Area——

(*f*) The decline in latitude is much less rapid, and is interrupted in both hemispheres, especially in the southern, by minor rises. Of these minor rises those in 1882 and 1892 for the southern hemisphere are most striking, as they cause the form of the southern latitude curve to resemble that of the northern area curve with its double summits; whilst the more regular latitude curve for the northern hemisphere seems to reproduce the sharply defined area

* That is the maximum for the Sun as a whole. This corresponds to the maximum of the southern hemisphere, and to midway between the two maxima of the northern.

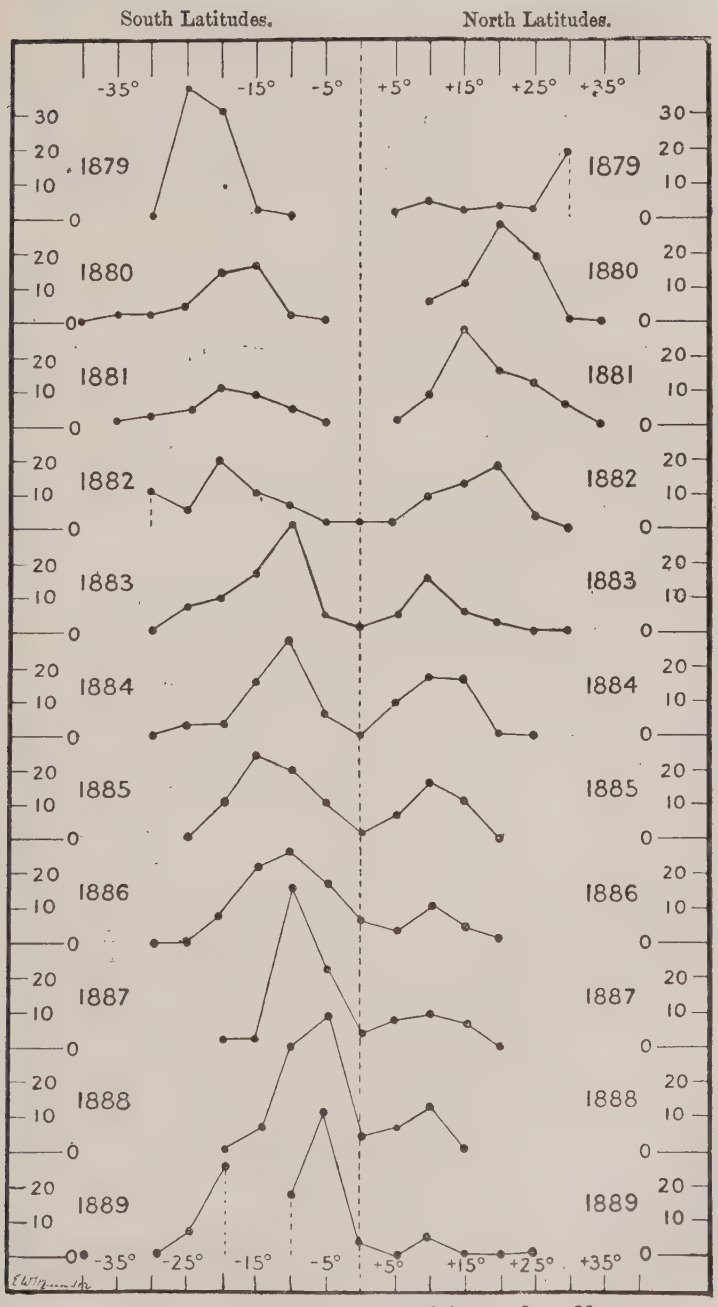


Fig. 4. Distribution of the Spotted Areas, 1879–1889.
The ordinates are percentages of the mean daily area for each several year

maxima of the south. In other words, the latitude curve attains in each cycle two maxima in the southern hemisphere, but only one in the northern.

(*g*) The mean latitude has fallen halfway to its lowest point by the time the mean maximum in area has been reached. As the decline in area after maximum takes place much more slowly than the increase in area before minimum the second half of the decline in latitude proceeds much more slowly than the first half.

(*h*) The range in latitude is greater in the northern hemisphere than in the southern, the mean position both rising higher and falling lower in the former case.

Fig. 4 shows the distribution of the spotted area, in zones of latitude 5° in breadth, for the years 1879 to 1889. The areas are expressed in percentages of the mean value for each several year, not on a uniform scale for all years of millionths of the Sun's visible hemisphere.

Fig. 5 gives the same results for the years 1890 to 1900. The two figures therefore practically give for these years the results shown in plate 15, vol. lxiii. of the *Monthly Notices*, but for 5° zones of latitude instead of 1° zones.

In some respects these two figures better represent the actual facts of spot distribution than the more detailed diagram just mentioned ; and for this reason. The 5000 groups of sun-spots of the Greenwich record may be divided into three great classes. The great majority of the groups may be described as "Undeveloped." They last only a short time—sometimes only a few hours, sometimes four or five days—they are small in size and never attain a great area, and they close up and disappear without going through the regular stages which mark the long-lived groups. The second class may be described as "Normal Spot-streams." These pass through a certain sequence of changes, which varies little in general character from one group to another. They begin as a pair of small spots, develop into a stream of considerable length, the first and last spots increase in size, the middle spots die out, then the rear spot breaks up, and the leader survives as a well-defined circular spot. The members of the class not only conform to a certain life-history, they also conform to a certain standard of length, breadth, and area. This standard changes during the progress of the spot-cycle, but the range of size shown by different members of the class is at no time great. The class is fairly long-lived, being generally seen in the course of three or more rotations. The third class may be described as "Abnormal" or "Giant Spot-groups." These are of great extent, often five or six times as large as groups of the second class ; they manifest some striking departures from the regular programme in the course of their development, and they vary very greatly as to their duration.

It is in the immense areas often attained by members of this third class that the explanation of the curious peaks and steeples

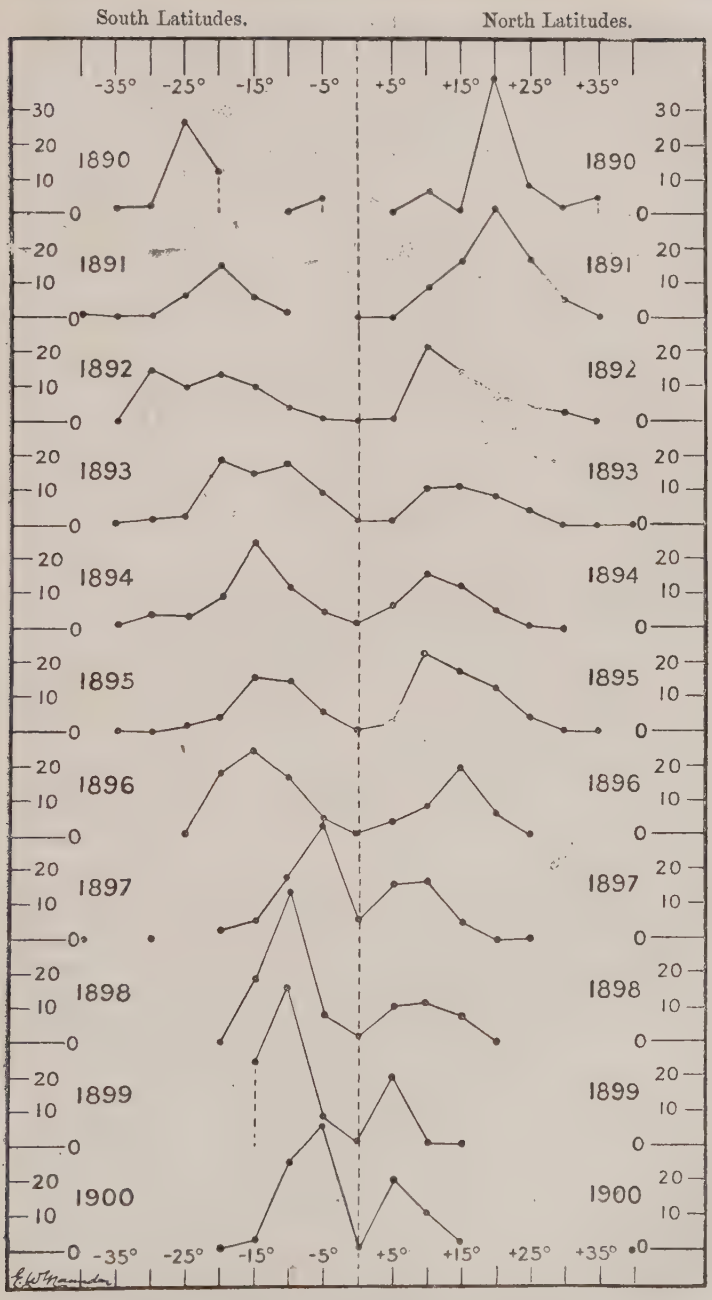


Fig. 5. Distribution of the Spotted Area, 1890–1900.
The ordinates are percentages of the mean daily area for each several year.

of the Greenwich diagram really lies ; for the breadth of these groups is usually 6° or 7° of solar latitude, sometimes even 10° ; but in forming the tables and diagram of the Greenwich paper of a year ago the entire area of any spot-group was referred of necessity to the 1° zone of latitude in which its "centre of gravity" was placed, inevitably causing a great apparent condensation in that zone—an *apparent* condensation which, if treated as a real one, must inevitably lead to mistaken conclusions. These peaks and spires in plate 15, vol. lxiii., are due, therefore, to isolated outbursts, not by any means to zones of activity, and are brought about by the purely computational concentration into single-degree zones of spot areas really distributed over many degrees. The feature is a feature of solar book-keeping, not of solar physics. So far is this the case that, though the plate in question represents the discussion of 5000 spot-groups, yet each one of the "Nineteen Largest Spot-groups" tabulated in my paper on the "Great Magnetic Storms" (*Monthly Notices*, vol. lxiv. No. 3) can be easily identified upon it. The majority of them stand out with an exceeding conspicuousness. More than this, so great a preponderance is exercised by some of these groups that their influence still shows out unmistakably even when the mean latitude is taken for an entire hemisphere and for a year at a time, even at solar maximum. Thus in fig. 3 the two second peaks of the latitude curve for the southern hemisphere are due to the two great groups Nos. 729 and 2421 in 1882 April and 1892 February respectively. Group No. 3412 in 1894 February creates one of the minor rises in the same curve. But their influence is not always in one direction. More usually the tendency of a great explosion is to seek a latitude a little higher than that proper to its period in the progress of the solar cycle. Occasionally, however, they appear distinctly below the average latitude, and then their influence is just as evident in producing a minor fall in the latitude curve. Thus in fig. 3 the notable drop in the latitude curve at the year 1894 is the effect of group No. 3668 of August of that year, and in fig. 4 the drop for 1897 of group No. 4497 early in January. The series of undulations in which, as Dr. Ch. Braun, of Kalócsa, pointed out, the latitude curve proceeds during its course of running down from one spot-minimum to the next is simply due to the influence of a very few of these "giant spots," which by reason of their size cannot fail, unless they chance to fall exactly on the mean curve, to drag the curve to the one side or the other.

The arrangement of spots into zones five degrees wide is, therefore, the most detailed possible if it is desired to exhibit their true distribution on the solar surface. An arrangement into zones two or three degrees wide is doubly objectionable. On the one hand it conceals the fact, so apparent when single zones are used, that the most salient irregularities are due to perfectly isolated outbursts, few in number and gigantic in scale, whilst, on the other hand, it gives rise to purely fictitious maxima, for it

concentrates into zones of two or three degrees spot-areas that on the Sun are actually spread over five, six, seven, or it may be even ten degrees of latitude. The maxima, therefore, obtained by Dr. Lockyer in the paper referred to above by taking 3° zones are not real maxima but merely apparent.

Turning to the curves shown in figs. 4 and 5, it will be seen that with 5° zones certain salient features of spot-distribution in latitude are clearly brought out.

(*i*) There is one and only one maximum, and that sharply defined, in each hemisphere in each year. This is the general rule.

Two classes of apparent exceptions are to be noted. In four instances, even taking the spots of a year, and grouping them in 5° zones, a single spot-group still succeeds in standing out. These are all in the southern hemisphere and are the following :—

Group	No. 729, 1882 April,	Lat.	−28°·5
„	„ 2421, 1892 February	„	−28·3
„	„ 3106, 1893 August	„	−17·6
„	„ 3281, 1893 November	„	− 5·1

The other instances are just after minimum, and are illustrations of "Spoerer's Law of Spot-zones"; for, as Spoerer pointed out, at this stage of the spot-cycle the first members of the new spot-cycle begin to appear in high latitudes, whilst the last members of the expiring spot-cycle have not yet wholly died out in the low latitudes to which they are confined. Thus in 1879 and in 1889 two clearly defined maxima are seen in both hemispheres, separated, as the more detailed diagram in plate 15 of the former Greenwich paper shows, by a broad zone absolutely free from any spots at all. In 1880 the old cycle-spots in low latitude have completely disappeared, and only a single maximum, and that sharply defined, is seen in each hemisphere. In 1890 the process of elimination of the old cycle spots is not quite complete, and they still show feebly; but the new spots in high latitudes are in a most overwhelming preponderance. It is possible to consider two or three very small spots of the following year as being survivors of the old cycle, but there is no possible question of a double maximum.

(*j*) The real exceptions to (*i*) occur, therefore, only immediately after the sun-spot minimum, precisely in accordance with "Spoerer's Law."

Some time ago Dr. Lockyer stated to this Society that "the curves of Spoerer are very misleading, for by taking the mean position of several spot-zones you arrive at a latitude in which spots may not exist at all" (*Observatory*, 1903 June, p. 236). This statement is true only respecting the commencement of the new cycle. Then, and only then, are there definite and distinct zones of spots in either hemisphere. But so far from

this being an exception to Spoerer's Law it is a relation which is especially brought out and insisted on by it.

At all other times there is practically but one spot-zone * in each hemisphere, and necessarily the mean latitude for the hemisphere falls not where spots do not exist at all, but where they are largest and most numerous. This can be seen at once to be the case from a comparison of figs. 2 to 5 inclusive, but is brought out again in a slightly different manner in fig. 8 (Plate 16). It is sufficient to state here that the careful examination of the photographic record of the last thirty years fails to show any fact to give the slightest support to the above statement of Dr. Lockyer.

Referring again to figs. 4 and 5 two other relations appear evident :—

(*k*) During the rise to maximum the spots avoid the equatorial region, but after maximum they gradually abandon the higher latitudes, and close in towards the equator.

(*l*) There is a strong tendency for the curves of any year in fig. 5 to reproduce the curves of the corresponding year in fig. 4—that is to say, eleven years earlier.

The reproduction is not quite exact, for, as the sun-spot cycle is not precisely eleven years, but varies from time to time, the cycle attaining its maximum in 1883 being shorter, whilst the following cycle was longer than the average, years separated by an undecennial period do not in this case represent precisely the same phases of the cycle. But even as it is the chief characteristic differences between the curves of the northern and southern hemispheres seen in the earlier cycle reappear in the later.

Fig. 6 exhibits the numbers of figs. 4 and 5 in a different manner. Here the areas for each zone 5° wide are treated separately, the annual means for that particular zone being shown in one and the same line, from 1874 to 1901. The areas are given in millionths of the Sun's visible hemisphere. Since, as already seen, the spots of either hemisphere form but a single zone this division is not a natural one, and conclusions must not be drawn from it without reference to the manner in which it has been formed. But certain relations do appear to be presented by it :—

(*m*) As pointed out in the Greenwich paper of a year ago, "spots in a higher latitude than 33° are at all times rare, and when seen are never large or long-lived. Taking them as a class by themselves they are seen irregularly, appearing at times which do not seem to bear any fixed relation to any one of the four chief stages of the sun-spot cycle—minimum, increase, maximum, and decline."

* I trust to be permitted in a future paper to deal with the distribution of spots both in latitude and in longitude. The above statement is absolutely correct, but I shall then be able to treat of certain restricted senses in which it is possible to speak of minor spot-zones.

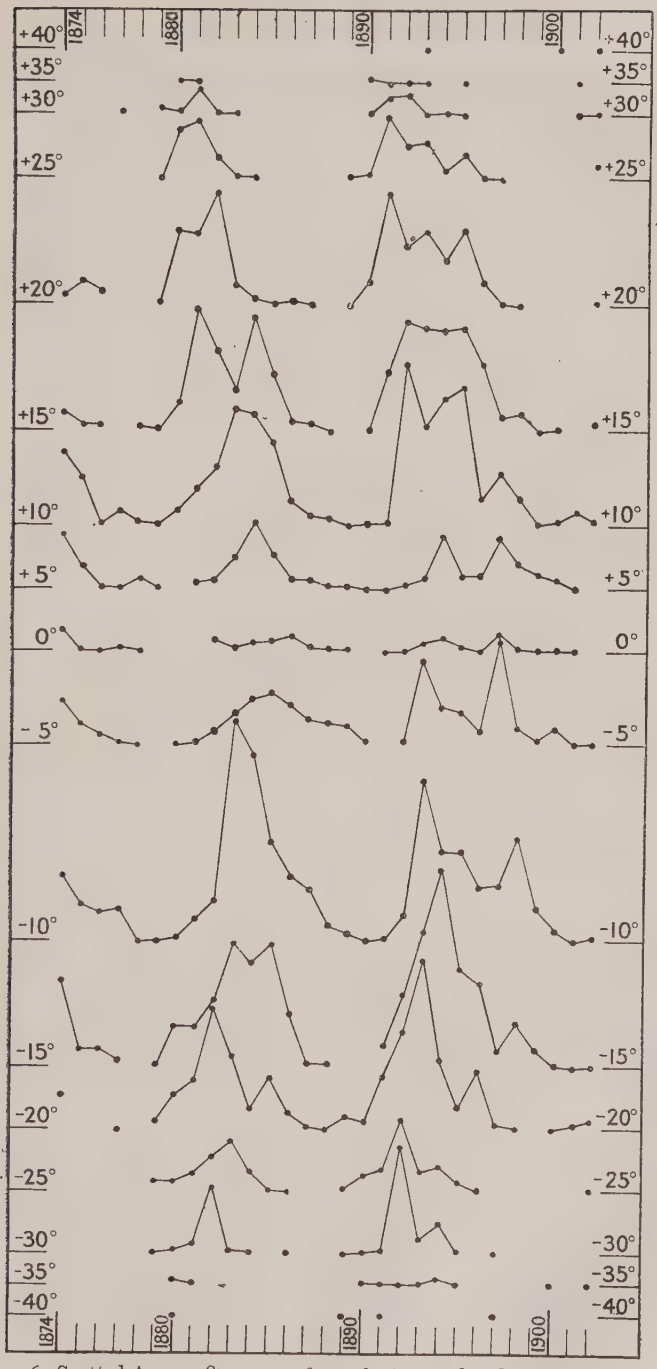


FIG. 6. Spotted Areas, 1874–1902, for each Zone of five degrees breadth in Latitude.

(*n*) Equatoreal spots are not, as is often stated, absent at maximum, but are fairly numerous. They commence to appear at, or a little before, maximum, and decline irregularly until their extinction early in the next cycle.

(*o*) The years of the maximum spot activity of the Sun taken as a whole are, broadly speaking, the years of maximum activity in every latitude, and not only in the zone of 15°. But the maxima for latitudes higher than 15° tend on the whole to fall earlier, and those for latitudes lower than 15° later, than the maxima for the 15° zone itself.

(*p*) The tendency of the northern hemisphere to reach the critical stages of the sun-spot cycle earlier than the southern is not only seen when the two hemispheres are treated as wholes, but, broadly speaking, is seen in every latitude.

Referring again to Dr. Lockyer's paper on "Sun-spot Variation in Latitude," the question arises whether even if the numerous maxima which he supposed in either hemisphere really existed, he would be justified in joining them together from year to year in the way he has done. Whilst it must be remembered that a spot of 150-millionths—and this is almost the smallest size for a group of the second class, the "Normal Spot-streams"—will have a breadth of 2° of solar latitude, and that a circular spot 1° in diameter will only have an area of 38-millionths, and will be only a small spot of the "Undeveloped" class, yet, on the other hand, immense as is the area of some spot-groups, the proportion of the solar surface covered by spots at any one time is quite insignificant. The mean daily area during the last maximum was one-seventh of 1 per cent., at minimum one-four-hundredth of 1 per cent. On no single day since the Greenwich record was commenced has the total spotted area reached 1 per cent., whilst the average for the whole period is about $\frac{1}{250}$ of 1 per cent. Further, the number of groups visible at any one time is, on the average, only slightly over one group for the southern half of the visible hemisphere, and a little less for the northern half. This being so, the barren areas between group and group must necessarily be very great as compared with the groups themselves, especially in the early days after a minimum, when the total spotted area is extremely small. Under such circumstances the linking together of certain selected spot-centres to form a "spot-activity track" can only be regarded as a matter of personal predilection, not as one of solar physics.

Fig. 7 will serve to indicate an example of this. It shows the distribution in heliographic latitude and in date of appearance of all the spot-centres in the northern hemisphere during the four years 1878 to 1881. The centres of 1878, eleven in number, all lie between +16° and the equator. A period of more than five months followed without any spots at all, and then in 1879 May a new group, No. 281, appeared in latitude +14°; a second, No. 283, in latitude +28°; and a third, in

latitude +6°, broke out in July ; and nine others followed before the end of the year. Following Spoerer's enunciation, the group at +28° was clearly the first of the new cycle, whilst that at 6° was as clearly a survivor of the cycle that had just passed ; but in which category should the first group, at +14°, be classed ?

Of the other groups of the year one at +18°, No. 289, may be mentioned. This and the group already referred to at +28° are considered by Dr. Lockyer as commencing two "spot-activity tracks." He writes : "Considering the curves relating to the Sun's northern hemisphere, it will be seen that in 1879, the year following a sun-spot minimum, when the spots were ending a cycle near the equator, two new outbreaks occurred in latitudes about 20° and 30°. These two centres of activity moved towards the equator next year, and by 1881 the former had disappeared, while the other rapidly grew in intensity and reached latitude 15°. During this year a new outbreak in latitude 30° made its appearance, and this in the two following years had an equatoreal trend."

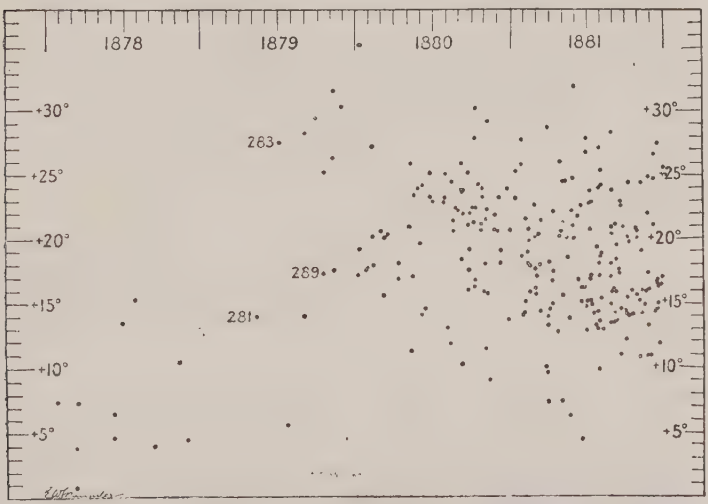


Fig. 7. Distribution in Latitude of Spot-centres of the Northern Hemisphere, 1878–1881.

An examination of the diagram (fig. 7) shows that this association of groups Nos. 283 and 289 with a downward moving current is purely arbitrary. The groups succeeding group No. 283 in or near the same latitude tend very markedly on the whole towards a higher latitude, not towards a lower. So with group No. 289. The first ten groups succeeding it in order of date are all either at the same or at higher latitudes, and it is not until five months after its appearance that another spot forms in

a slightly lower latitude. There is even some slight evidence of an actual movement upward of group No. 289, for in the next rotation group No. 293 forms in the same longitude, but very slightly higher in latitude. Other groups early in 1880 show the same tendency. Group No. 308 is an actual return of group No. 301 with an increase of 0°·8 in latitude ; group No. 320 a second return of group No. 314, with an increase of 0°·7 in latitude. A careful examination of the diagram shows, in fact, that the real phenomenon that was in progress in 1880 and 1881 was by no means the setting up of downward moving "spot-activity tracks," but a general multiplication of spot activity in all latitudes consequent upon and concurrent with the general increase in the spotted area.

There is, therefore, no satisfactory evidence in favour of the existence of Dr. Lockyer's downward moving "spot-activity tracks." The last diagram gives positive evidence against it.

Fig. 8 (Plate 16) displays for the entire period 1877–1902 the same features shown in fig. 7 for the four years 1878–1881. But in this case, in order to bring the facts within reasonable compass, instead of the individual spot-centres being represented, the spot-distribution for each synodic rotation of the sun has been given. Wherever a spot-centre has fallen on one or more days during a given rotation in a particular degree of latitude a line has been drawn across that particular degree. The diagram, therefore, like fig. 7, represents distribution in latitude, but takes no account of area. In assigning a spot-centre to any particular degree of latitude all latitudes from, say, 6°·5 to 7°·4 have been reckoned as 7°, and similarly with others.

An examination of the diagram brings out Spoerer's Law with remarkable clearness. There is but a single movement in either hemisphere, the general trend of which is downward from immediately after minimum to immediately before minimum. During the years of great spot-activity almost every degree of latitude is affected. The interruptions are perfectly irregular and sporadic. The indications of subordinate zones, which would be shown, if present, by continuous barren tracks, through the middle of the vertical lines, are quite wanting. Nothing can be more striking than the contrast between the very fair approach to continuity of the distribution lines, during the greater part of the solar cycle, and the definiteness with which they are limited, both on the side of the equator and on that of high latitudes, by barren belts descending towards the equator. The general form of spot-distribution in both cycles is that of a hollow wedge.

But there are some minor details which are worth notice. A comparison of both cycles and in both hemispheres shows that though the outbreak after minimum occurs quite suddenly in high latitudes it does not at once start in the highest latitude, and then run down. There is a short time, in length about a year or a year and a half, during which the outer boundary of the

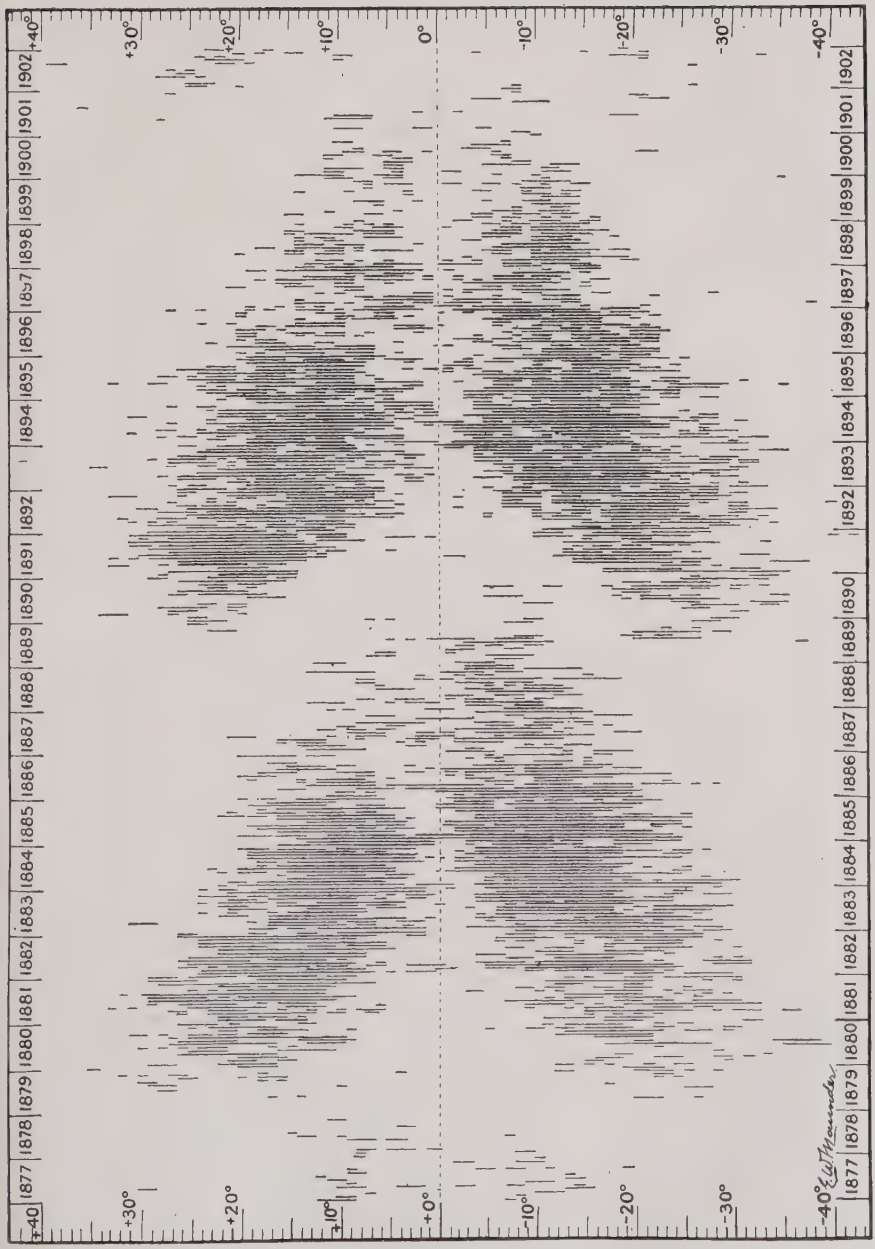


FIG. 8.—DISTRIBUTION OF SPOT-CENTRES IN LATITUDE, ROTATION BY ROTATION, 1877–1902.

spot-distribution is steadily and continuously mounting. Fig. 7 shows this in detail for the northern half of the first cycle, group No. 283 and its successors moving steadily upwards. Inasmuch as the general increase in spot activity is eventually seen in all zones there must be a broadening of the general area of disturbance, and therefore it might be expected there would always be a few spots which could be selected in order to favour the idea of short-lived downward currents. In 1889 in the southern hemisphere, however, the current moved upwards, and upwards only. There were no spots for a year to offer any excuse for the idea of a downward moving "spot-activity track." In the previous cycle a similar feature was presented, but was not quite so strongly marked. The limit, therefore, of spot-distribution in latitude varies in precise analogy with the variation in area—that is to say, it shows a swift run up to maximum, a slow and undulating movement down to minimum.

Though the diagram shows clearly that there is but a single spot-zone in either hemisphere in each of these two cycles, a zone which moves in general accordance with Spoerer's curves, it reveals a striking and unexpected fact—namely, that the southern current not only reaches the equator, but crosses it. The limit which bounds spot-distribution in the southern hemisphere on the equatorial side can be traced not only as far as the equator, but beyond it.

Fig. 8 therefore shows :

(*q*) That Spoerer's Law is completely true within the limits of its enunciation.

(*r*) That there is only one spot-zone—a very broad one—in either hemisphere except during the brief period just after minimum, when two cycles overlap.

(*s*) That Spoerer's Law is defective in so far as it does not take note of the short period during which the limit of spot-distribution moves upward at the beginning of a new cycle.

(*t*) That it is also defective in not taking note that the downward movement of the limits of spot-distribution may, as in 1886 *et seq.*, lead one of the two spot-zones right across the equator.

On the whole the results of the Greenwich record for the twenty-nine years 1874 to 1902, briefly summarised on pp. 452 and 453 of the *Monthly Notices*, vol. lxiii., fully confirm and bear out "Spoerer's Law of Spot-zones," whilst indicating some important additions to it.

86 *Tyrwhitt Road, St. John's, S.E.:*
1904 *June* 9.

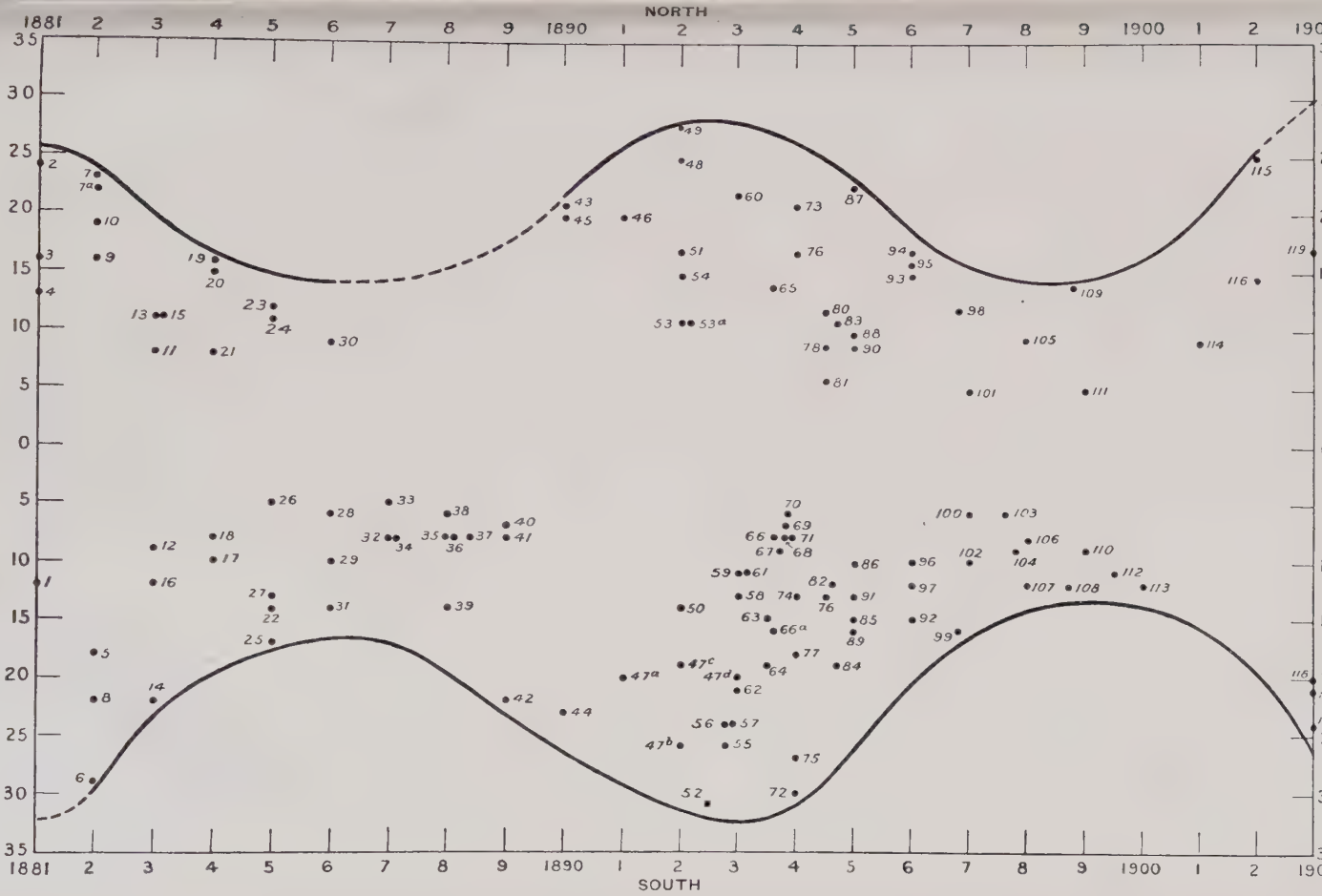
Variation in Latitude of the Greater Sun-spot Disturbances, 1881–1903. By the Rev. A. L. Cortie, S.J.

A former paper (*Monthly Notices R.A.S.*, lx. No. 8) contains a list of 115 greater sun-spot disturbances drawn at Stonyhurst during the years 1881–1899, as also a brief description of the life-histories of the same groups, covering a period of 255 solar rotations. In general, by a greater disturbance is meant either a single spot or group of spots, the area of which amounted to $\frac{1}{1000}$ of the Sun's visible disc, returns of the same spot or group being reckoned as a single disturbance. A few of the groups admitted into the list, although not attaining the disc-area required, were either recurrences, as distinguished from returns, of these groups, or in some special way connected with them. To make the record complete to the end of the year 1903, eight other groups have been added to the list, two from the Greenwich volumes for the years 1900 and 1901, and six more for 1902 and 1903, of which the heliographic latitudes have been very kindly furnished me by Mr. Maunder.* The total number of disturbances therefore discussed is 123 for the period of twenty-three years during which solar observations have been continuously made at Stonyhurst.

In view of recent criticisms on the absolute validity of Spörer's law regarding the variation of the zones of sun-spot activity with the progress of the sun-spot cycle, a chart (Plate 17) has been prepared to accompany this paper, on which the mean heliographic latitudes of the greater and associated disturbances have been plotted for each year as a series of points, with a number attached to each referring to the list of these disturbances before published (*loc. cit.* pp. 532–535). For such disturbances may legitimately be regarded as indices of the chief foci of solar activity, and any progressive shiftings in the zones of sun-spots, if it exists, should be chiefly reflected in them. That the inclusion of smaller disturbances on the chart would not materially alter the zones of sun-spot activity has been ascertained by a comparison with the diagram containing the mean daily area of sun-spots for each degree of solar latitude for the period 1874–1902 as measured on the Greenwich photographs, and published with accompanying tables (*Monthly Notices R.A.S.*, lxiii. No. 8).

Spörer's law of sun-spot variation in latitude contains two main propositions, the one, that the mean latitude of sun-spots diminishes gradually from $\pm 30°$ at times of solar maximum to $\pm 5°$ at times of minimum; the other, that before a sun-spot cycle is completed a second begins abruptly by the appearance of sun-spots in high latitudes. His words are: "A partir du mini-

* These eight disturbances are numbered 123–130 on the chart, the Greenwich group numbers being 4915, 4953, 4968, 4990, 5093, 5098, 5100, 5104.



HELIOGRAPHIC-LATITUDE CHART OF THE GREATER SOLAR SPOTS, 1881-1903.

mum, les taches, qui avaient depuis longtemps déserté les hautes latitudes, s'y montrent brusquement vers $\pm 30°$" (*Comptes Rendus*, cviii. 486). The process, therefore, is that of a succession of overlapping waves of activity descending from higher to lower latitudes. All solar observers will be agreed as to the general correctness of the first proposition. An inspection of Spörer's own charts, published in the Potsdam Observatory volumes, is sufficiently convincing on this point. At the same time all will equally admit that a mean latitude curve of sun-spots drawn for each solar hemisphere north and south of the equator is not a continuous curve, but that while on the whole the decline in latitude is gradual and continual, it proceeds in a series of waves. This is well shown in the curve drawn by Father Braun from observations taken at Kalócsa during the period 1880–1884, and reproduced in Dr. Lockyer's paper on this subject (*Proc. R.S.*, vol. lxxiii., and *Monthly Notices*, Appendix to vol. lxxiii.). In this paper, however, Dr. Lockyer comes to the conclusion that Spörer's law gives only a very general idea of sun-spot circulation, and that the curves are but the integrated result of a series of well-defined spot-tracks which are analogous to atmospheric storm-tracks on our globe. These "spot-activity tracks," according to Dr. Lockyer, commence successively in higher latitudes as the maximum is approached, and then fall nearly continuously in latitude. Instead, therefore, of two large overlapping waves, the action would be a succession of smaller waves or tracks; and whereas, according to Spörer, a large mean latitude wave falls continuously from higher latitudes at maximum to low latitudes at minimum, the starting points of Dr. Lockyer's tracks increase in latitude as maximum is approached. Still they all tend downwards, their deviations being from higher to lower latitudes. But it would appear from a study of the mean heliographic latitudes of the greater disturbances for the period under discussion, that the wave of sun-spot activity is highest at maximum, descends as minimum is approached, and after minimum that the direction is polewards until a second maximum is attained. This hypothesis would substitute for Spörer's large overlapping high to low latitude waves and Dr. Lockyer's series of "spot-activity tracks" a wave which falls from maximum to minimum and then rises. In so far as Dr. Lockyer places the successive waves with their starting points in higher and higher latitudes as maximum is approached, this suggested wave of "limiting sun-spot latitude" would pass through his successive points, but would disagree in making the trend of activity polewards from minimum to maximum, instead of towards the equator. It would contravene, too, the second main proposition of Spörer's law. But it would bring the variation of latitude in the case of greater spot disturbances into accord with the general poleward tendencies of the centres of prominence action from minimum to maximum, and the gradual opening out of the great coronal streamers as maximum is approached. At times of

solar maximum, as for example in 1893, nearly every latitude of the solar surface is disturbed with spots, extending from −6° to −30° in the example quoted. This general disturbance affects the prominences and corona and all other phenomena indicative of solar activity. Hence it is that prominences are found in the polar regions at such seasons of solar unrest, and that the coronal streamers are so widely diffused round the solar disc. At such times magnetic storms are more frequent and violent upon the Earth, pointing to a connection with the generally disturbed state of the Sun, rather than with isolated phenomena as spots, or faculæ, or polar prominences, all attempts to connect magnetic storms with such particular phenomena having so far proved unsuccessful (*cf.* "On the Connexion between Solar Spots and Earth-magnetic Storms," Sidgreaves, *Memoirs R.A.S.*, vol. liv. "Minimum Sun-spots and Terrestrial Magnetism," Cortie, *Astro-physical Journal*, vol. xvi. No. 4; and "Solar Prominences and Terrestrial Magnetism," Cortie, *ibid.* vol. xviii. No. 4). Nor is there any radical change in the constitution of the spots at various epochs of a sun-spot cycle, so far at least as can be gathered from my own observations of the red-end spectrum of sun-spots extending intermittently over a period of twenty years. But on this point my observations are contravened, at least in the earlier years, by the results from South Kensington obtained in the yellow-blue region of the spectrum. However, whether or not the character of the spots changes with the progress of the cycle from minimum to maximum, there seems to be sufficient evidence to show a distinct movement to higher latitudes when the spots become more frequent and larger.

From the chart it will be seen at once that during the period discussed the greater disturbances were much more frequent in the southern than in the northern hemisphere, though the law of variation in latitude is equally shown in both. Taking the southern hemisphere, spots of this class declined in latitude from −29° in the maximum year 1882 to −5° in 1887, but then began to go polewards again, No. 39 in the last quarter of 1888 being in latitude −14°, No. 42 in the middle months of 1889 being in latitude −22°, followed by No. 44 in latitude −23° in the last three months of 1890, and culminating in No. 52 of 1892 June, about the time of maximum, in latitude −31°. Similarly, in the northern hemisphere, Nos. 43 and 45 of 1890 in latitudes +21° and +20° culminated in No. 49 of 1892 March in latitude +28°. In the next cycle the latitudes in the southern hemisphere descended from −16° for No. 99 of 1896 November to −8° for No. 106 of 1898 January and −9° for No. 110 of 1899 March, and then began to rise, No. 113 in 1900 being at −12°, and Nos. 117, 118, and 120 of 1903 being between latitudes −20° and −24°. A corresponding rise took place in the northern hemisphere from No. 109 of 1898 August in latitude +14° to No. 115 of March 1902 in latitude +25°. It is to be noticed that, the foci of activity having reached ±20° in the

years 1889–90, disturbances about the same latitude continued to occur until the end of 1895, though they ceased in the belt −25°–30° for the southern hemisphere in 1894 May and for the northern hemisphere after 1892 March. Spörer has already indicated the mean latitudes ±20° as the seats of great disturbances about the times of maximum. At the times of maximum too not only are greater disturbances found in high latitude zones, but in all zones. For instance, the belt at mean latitude −8° S., which had shown many signs of activity during the minimum years 1887–1889, again became active in 1893 August, a maximum year, no fewer than six greater disturbances occurring in a belt between 1893 August and 1894 February. The same belt or zone was not again active until 1897–1899, after which the greater disturbances began to go polewards once more. Very remarkable too is the persistence of a region of disturbance between the limits −20°–26° from 1891 September to 1893 March, the groups being numbered on the chart 47*a*, 47*b*, 47*c*, and 47*d*, their life-histories showing that they were all linked by means of smaller disturbances in the same region. The great magnetic storm of 1892 February was connected with this persistent activity, as indicated by the spot-groups. Two smoothed curves have been drawn on the chart to indicate the nature of the variation in latitude, as shown by the greater disturbances, and these have been called curves of "limiting latitude." The dotted portion of the curve for the northern hemisphere denotes a dearth of points to settle the trend of the curve for the period 1886–1890, though the fall from 1882 to 1886 and the rise subsequently to 1890 is very evident, and similarly for the southern hemisphere in 1900–2.

In Dr. Lockyer's paper (*loc. cit.*) the "spot-activity tracks" are formed by joining together successive centres of sun-spot activity, such as are tabulated and shown diagrammatically in the Greenwich results already referred to, the observations being grouped in strips of latitude 3° in width. Assuming a general trend of sun-spot activity towards the equator from the poles, such as is demanded by Carrington and Spörer, and given a number of centres of activity, it is evident that such tracks can be formed by joining together the different centres of activity. But the validity of this interpretation of the variation of the spots in latitude may be questioned when it is observed that various centres of activity that ascend from lower to higher latitudes, from minimum to maximum, are taken as the starting-points of tracks which are descending. Would it not be at least equally legitimate to make a series of ascending tracks at such epochs, which would be more consonant with the trend of the centres of prominence activity? Again, although there is distinct evidence of various latitude zones becoming intermittently active at different times in a spot cycle, and prescinding from the fact that in order to form "spot-activity tracks" it is necessary to join together spots that differ widely in longitude

and have only a fortuitous connection of latitude with one another, such tracks would have to include regions which are either disturbed for a long time or regions which are free from spots for a long time. For instance, the disturbances numbered 47*a*, 47*b*, 47*c*, 47*d* on the chart are all parts of one great disturbance which was located in that particular six degrees of latitude for 527 days. The disturbances in this position at least, which included some of the greatest of the last maximum, cannot be isolated the one from the other to form different points in various tracks. In Dr. Lockyer's diagram three tracks occur during this period.

In the paper (*Monthly Notices R.A.S.*, lxiii. No. 8) containing the Greenwich results it is noticed that "at minimum each hemisphere, considered separately, showed two clearly defined spot-zones marked off from each other by a broad belt in which there were no spots at all. This was especially marked in the years 1889 and 1890, when the very region, centering about latitude 15°, which when an entire solar cycle is considered is the most prolific of the whole solar surface, was completely free from spots." The same fact with regard to the greater disturbances is also shown on the chart, and the interpretation suggested is that the new cycle is beginning by the ascent of a series of disturbances to higher latitudes, which culminate at the maximum. Meanwhile the former cycle is dying out in low latitudes, and thus the central regions are left bare of spots. This dearth of spots in central regions is not so marked at the succeeding minimum. These facts, however, as to the persistence of disturbance in definite regions at some epochs and dearth of spots at others do not lend much countenance to the view of the variation in latitude being effected by a series of "spot-activity tracks." Is it not rather a matter of intermittent action in belts or zones? In this connection the series of disturbances in the latter half of 1893, numbered 66–71, which formed a belt in latitude −6° to −9° is noteworthy.

The following conclusions would seem to be warranted by the facts adduced :—

1. Greater disturbances are most prevalent in high latitudes at or near the times of solar maximum ; they fall to middle and lower latitudes during the phase of decline, but again begin to seek higher latitudes at the minimum and period of increase.

2. The curve of limiting latitudes for greater disturbances reproduces very faithfully the form of the curve of total spotted area.

3. The process of decline is for spots to disappear first in the higher zones and then in the middle zones, while the lower zones may be affected equally at minimum and maximum.

4. There does not seem to be any indubitable evidence of an abrupt commencement of a new cycle by the appearance of spots in high latitudes.

5. The process of spot variation in latitude does not seem to

be due to the overlapping of cycles of activity, as demanded by Spörer's law.

6. Nor is it apparently due to the existence of a series of "spot-activity tracks," the subsidence of activity from maximum to minimum being zonal and the foci of activity in the zones occurring in different longitudes.

The Rotation Period of Saturn. By W. F. Denning.

The conspicuous spots visible on *Saturn* in 1903 offered an opportunity for accurately determining the rotation period of the planet which it was most desirable to utilise to the best possible advantage. I regretted, therefore, to see that Professor Hough had apparently misidentified certain of the markings, and deduced a rotation period in excess of the correct value (*Monthly Notices*, 1904 December).

Professor Hough places an exclusive reliance on the micrometric method of taking transits, and gives the times to tenths of a minute, but the number of the observations he employs are so limited that at least very grave doubts must exist as to his identifications. He regards all eye-estimated transits of markings on *Jupiter* and *Saturn* as useless, though Professor Barnard, Mr. Stanley Williams (*Monthly Notices*, 1904 March), and others have clearly shown that the latter are comparable with the former in regard to accuracy.

It is indeed quite fair to say that if all the micrometrically measured transits of *Jupiter's* spots were put on one side, and only the eye-estimated transits retained, our knowledge of Jovian phenomena would not suffer in the least!

The conclusions I arrived at respecting the identification and rotation period of the spots on *Saturn* (*Monthly Notices*, 1904 March) are, I believe, perfectly sound, and this is also the opinion of several well-qualified observers who have not only obtained transits, but have pretty thoroughly compared and investigated the details.

As I have previously stated, the identification of the individual objects is a point of greater importance than the method of taking transits; and to be certain of the correctness of the identifications a fair number of observations must be accumulated. The materials adduced by Professor Hough in his paper (*Monthly Notices*, 1903 December) are obviously too scanty for the individual spots to be followed with certainty. The spots were quite numerous, some of them pretty close together, and liable to variation in brightness, shape, and motion. Professor Hough relies upon an observation, after an interval of three weeks, of a "faint, difficult" spot on August 19 as being the same as Barnard's, though it was more than 5,000 miles nearer the

equator, very much smaller than the object named, and (as I have endeavoured to prove in *Monthly Notices*, January 1904, p. 241) 25°, or 45^m, east of it.

I must confess myself surprised at several of the statements made by Professor Hough, but will content myself with particular reference to one. He says (*Monthly Notices*, 1904, p. 552) that my "suggestion that discordant observations may be due to oscillation in position of the spots on *Jupiter* and *Saturn* is not sound." Now Professor Hough himself has proved by micrometric measures the oscillation of the spots on *Jupiter* ! In his annual report of the Dearborn Observatory, 1882, p. 50, he distinctly remarks of a large white spot on *Jupiter* that, "observed continuously for a period of 252 days, it indicated sudden deviations in its apparent place, probably due to changes of shape. The comparison with the ephemeris shows a maximum displacement of 16^m, or more than $3''$ at mean distance." The apparently irregular movement of the markings on *Jupiter* (to whatever cause it may be due) is a fact well known to all students of the planet, and it is highly probable (if indeed it was not sufficiently proved by the observations in 1903) that the same thing occurs on *Saturn*.

I hope to return to this subject with greater detail, but fear there is little prospect of unanimity between Professor Hough and other observers so long as he holds his present views on the various methods of taking transits.

Bishopston, Bristol :
1904 *May* 20.

Note on the Gyroscopic Collimator of Admiral Fleuriais.

By M. E. J. Gheury.

Admiral Fleuriais's Gyroscopic Collimator is not by any means new, and although it has been greatly improved lately by Messrs. Ponthus and Therrode, of Paris, who kindly lent me one of their latest patterns, these improvements have left unaltered the characteristic features of the original collimator, being chiefly concerned in making the apparatus practical and easy in working, while in the hand of a skilful observer the results obtained with the latest form are of remarkable accuracy.

However, although a very interesting and elegant solution of the problem that sprung up with the requirements of modern navigation, the importance of which cannot be overrated, it is, I find, very little known either by seamen or by non-professional people interested in science generally. This is a pity, seeing the ingenuity displayed in overcoming the many obstacles preventing the realisation of a reliable artificial horizon.

Observations at sea differ from those on land in this respect : while on land the utmost accuracy is but a question of meteorological conditions, at sea considerations of a mechanical nature, even with the most favourable weather, prevent the obtainment of more than an approximation. Indeed, a greater exactitude would not be greatly beneficial ; one cannot be certain of the speed of a tide current more closely than within half a mile or so—even a good deal of experience is needed to ascertain it as closely—and no refinement in navigating methods would justify the captain of a ship to pass a rock at a distance of but a hundred yards, on pretence that he knows his position with mathematical accuracy. Where rocks are to be found tidal streams generally prevail, and in the best conditions a quarter of a mile is the closest berth the ablest navigator dares to give them.

Taking into consideration the various factors that enter into the determination of the position of a ship at sea, it may safely be said that to know that this position is within a circle on the chart of half a mile radius, or, to be more correct, and keeping in view the coordinate nature of the data, within a square the side of which is a nautical mile, is all that is required, and it is rare to obtain with certainty a smaller area of position, except in the neighbourhood of land, in calm, clear weather, by station-pointer observations of some selected landmark.

The sea horizon gives the above approximation in fine weather, when there is no abnormal refraction—a fact of which one is not generally aware when taking the observation. It fails entirely when the reflection of the Sun falls on the sea line, in misty weather, and at night, except in few special cases, it is at best undefined. Although whenever it is available it answers all the requirements of navigation, in many cases the opportunity of a good "fix" allowed by a clear day sky or a starry night is lost or gives but a very rough indication of position, because of the want of a reliable horizon.

I shall not refer to the various appliances that can be used to obtain this line of reference, of paramount importance to the navigator. I have tried several of them, and found them to be either unreliable or to need such preparations and favourable state of the weather as to be unpractical. Amongst the unsuccessful attempts to solve the problem I will, however, mention two, which have a special interest with regard to the Gyroscopic Collimator. One, invented, I believe, some sixty years ago, consisted in a horizontal mirror fixed on the top of a revolving gyroscope. The very nature of the support was the cause of failure, as the axis of such a revolving body is rarely vertical. The other was a pendulum, altitudes being taken with regard to a horizontal line marked on the bob when the latter was at three consecutive positions—at the beginning, middle, and end of a double oscillation. The altitude was then calculated for the mean position of the mark. This may be realised with a fixed point of suspension, but at sea such a point is nowhere to be

found, and the forced oscillations introduced by the rolling of the ship had such a large influence as to entail a prohibitive length for the pendulum in order to have a period of oscillation sufficiently large compared with that of the rolling of the vessel not to be disturbed by the motion of the latter.

In the Gyroscopic Collimator the motion of precession, which was the cause of failure of the first type I just mentioned, is taken advantage of to obtain a pendulum of very small size, the time of oscillation of which is very large, by the similarity existing between the motion of a pendulum and that of the axis of the gyroscope on each side of a vertical plane through the pivot.

This instrument is used in connection with an ordinary sextant, the observation being taken, as with the sea horizon, by bringing the image of the observed body in a field of vision in which a horizontal grating of a special kind allows the observer to ascertain the position of the sensible horizon and the angular distance the image is from it.

FIG. 1.

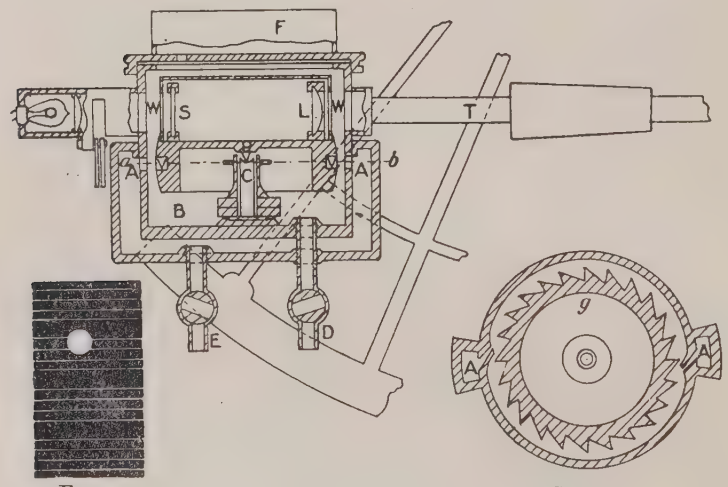


FIG. 3.

FIG. 2.
Section through *a b*.

At the centre of an air-tight case B (fig. 1) is fixed a small hemispherical cup C of hard material, the diameter of which is ·1″. This supports the pivot of the gyroscope, the latter having the shape of a spherical segment, the centre of which is coincident with the point of the pivot, while its centre of gravity is a little below this point, about ·02″. This segment is hollow, to combine lightness with a large moment of inertia with regard to the axis. Along its equator, in a groove V, are placed little vanes (fig. 2), by means of which the gyroscope is set in motion

by the inflow of air from the apertures A in communication with the atmosphere at E, when air is pumped out of the case B by the tube D.

On the top of the gyroscope is the optical apparatus giving the sensible horizon. This consists in a plano-convex lens L and a field grating S, placed at the opposite extremities of a same diameter, the grating, which is a succession of equidistant transparent lines perpendicular to the axis of rotation, on a black background (fig. 3), being at the focus of the lens.

The axis of this optical system is in the same horizontal plane as the line of sight of the observing telescope, the case B having two windows W facing each other along the line of sight, so that the light of an electric lamp, or of the Sun—reflected, in the latter case, in a small mirror fixed on the case—can pass through the latter and be received by the telescope whenever the axis of the optical system fixed on the gyroscope is coincident with the vertical plane through the collimation line of the telescope ; this occurs at every half-revolution. As the normal speed is about 100 revolutions per second a persistent image of the grating is observed in bright lines on a dark background, on which the image of the observed body can be brought down.

When a top so supported is made to revolve round its axis, this axis describes a cone of decreasing amplitude, and a point on it will describe a closing spiral. If the eye is at about the level of the pivot, and in a fixed vertical plane through it, the axis will be seen to oscillate on each side of the vertical, exactly as a pendulum, the amplitude of the oscillations decreasing gradually. By decreasing the distance of the centre of gravity from the pivot, or increasing the speed of rotation, the time of a cycle of precession can be increased, so as to minimise the influence of the motion of the vessel. We have thus a short pendulum with the time of oscillation of a very long one. This motion of precession is really made up of a succession of minute loops, similar to the loops described by the Earth in its nutation, but, with a sufficient speed of rotation, these loops are too small to be detected by the eye, as it is the point of the pivot, and not the centre of gravity, which describes them.

When the axis of the gyroscope is vertical the middle of the dark space of the grating is the trace of the sensible horizon in the field, a constant instrumental error $\pm e$, made up of two parts, (*a*) the instrumental error of the sextant itself, and (*b*) the collimation error of the gyroscope, being allowed for. The axis, however, is rarely vertical, and its spiral motion will give a balancing motion to the lines of the grating. When the axis of rotation is in the plane of the sextant the lines are horizontal and parallel to a spider thread in the telescope, perpendicular to the plane of the sextant, the centre line being below or above the plane of the sensible horizon, according to the position of the axis, inclined towards or away from the observer. As the gyroscope rotates the lines become slightly inclined to the horizontal,

becoming horizontal again after half a cycle, and so on. If we only consider the grating when its lines are horizontal there is then, at the successive positions of the gyroscope at which this occurs, a vertical oscillation of the lines, while the reflected image of the observed body will be comparatively steady, and will seem therefore to oscillate vertically with regard to the grating. A little spring, not shown in the figure, and acted upon by a lever, allows a slight pressure to be exerted upon the gyroscope to give it the necessary inclination for this precessing motion.

When the gyroscope has attained its full speed the tube E is closed, and two or three strokes of the pump are given, so as to produce a certain vacuum (about 6 cms. of mercury), shown by the gauge F fixed on the top of the case B. The tube D is then closed, the pump disconnected, and the instrument is available for observations for a lapse of time of about half an hour.

The image of the observed celestial body being brought down on the image of the grating, its position with regard to the centre line is observed at each reversal of its motion, when the grating is horizontal. A little practice will enable the observer to estimate the tenths of the intervals between the consecutive lines. The time corresponding to each separate observation of the image is taken by an assistant.

Means of series of observations made upon a collimator, to study the behaviour of the instrument, have given evidence of systematic errors, varying with the "launching" without apparent reasons, and giving a maximum deviation of $2'$ from the general mean. In good conditions the errors of individual observations differ but slightly from the mean of each series. This is sufficient to enable the instrument to be very useful at sea. The wear and tear of the cup supporting the pivot seem to be the principal causes of the observed anomalies.

On fig. 3 we see the position of the lower limb of the Sun to be $-50'$, each space being $10'$, and the telescope having an astronomical eye-piece. The positions of the star, from top to bottom, are $-71'$, $-46'$, and $+34'$. The space between the lines is not always $10'$, but is sometimes a little more or less; the readings must be therefore multiplied by a constant to get the true angular distance.*

There are several ways of taking an observation. A first method is to observe three successive positions at which the observed body changes its motion—that is, a maximum included between two minima or two maxima on each side of a minimum. Then if a', a'', a''' are the three positions in chronological order they are respectively on fig. 3 -71, $+34$, -46, and the correct reading a is

$$a = \tfrac{1}{4}(a' + 2a'' + a''') \text{ or here } \tfrac{1}{4}(-71 + 68 - 46) = -12'.$$

* For the instrument I have on trial the line space is $11'\ 30''$, the reducing constant being 1·15.

From these three readings and the time of a cycle of precession, given by the interval between them, a correction I shall consider later on, due to the motion of the Earth, is calculated, together with the altitude corresponding to the motion of the gyroscope when the axis is vertical. In other words, the effect of the rotation of the Earth and of the erecting tendency of the gyroscope must be ascertained.

To observe well-defined maxima and minima the precession must have a certain minimum value, especially for the observation of a celestial body out of the meridian, the movement in altitude of which may attain 15″ per second of time when on the prime vertical. Another method is to take a succession of readings as near each other as possible, either when the precession has almost ceased or with the full precession, as for the maxima and minima method. These, plotted with the time, will give in the first case almost a straight line from which the readings corresponding to any time may be taken; and, in the second, a wavy line, on which the maxima and minima can be selected and the corresponding times taken from the curve.

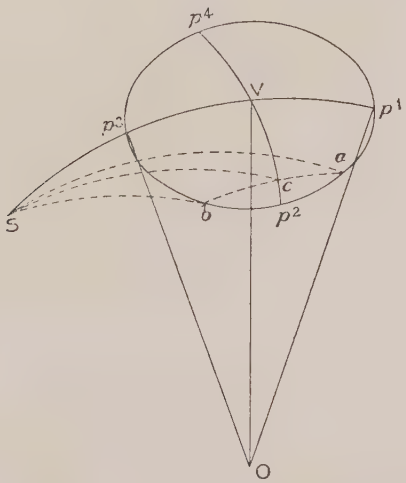


Fig 4.

When the celestial body is motionless, as it is approximately in a meridian observation, we can either take the mean of the three readings, and then we have Obs. Alt. $A = Ai + \frac{1}{4}(a' + 2a'' + a''')$, where Ai is the reading on the limb corrected for instrumental error—that is, $Ai = Ao \pm e$, Ao being the reading on the limb—or we can apply to any of the three readings a correction to bring it to what it would have been had the axis of the gyroscope been vertical by calculating the coefficient k of tendency to

verticality of the axis. In an observation out of the meridian, if the motion of the celestial body S in its vertical is larger than the motion of translation of the pole P of the gyroscope, the distance from S to P will increase continuously, and it will be impossible to have maxima and minima. Only if the translation of the pole is greater than the motion of the celestial body will the observation be possible. The theory of the instrument is rather long and complicated.*

If the translation of the pole P is greater than that of the celestial body, from p_1 to a the distance SP will increase, as at p_1 the speed of approach of P towards S is zero ; if a and b are the points for which the translation of the pole projected on the diameter p_1p_3 is equal to the motion of the body on its vertical, at a and b SP will be constant, between a and b SP will decrease, and on the rest of the path $bp_3p_4p_1a$ SP will increase again (fig. 4). The greater the speed of P compared to that of S the nearer a and b will be to p_1 and p_3 ; if ap_1 and bp_3 are very small, $\frac{1}{2}(Sa+Sb)=Sc$ will be equal to SV nearly.

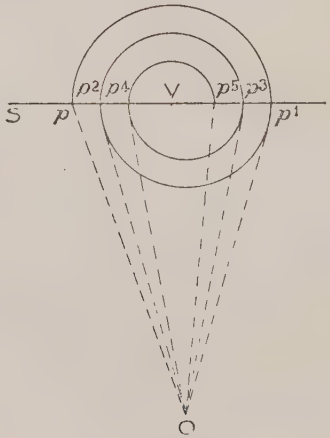


FIG. 5.

The speed of P is proportional to the radius of the path of precession—that is, to the inclination of the axis—the greater this inclination, the less will be ap_1 and bp_3. It can be shown that a minimum inclination of 1° will be quite sufficient if the minimum permissible error in SV be 30″.

If the point of the pivot is hemispherical the locus of a point on the axis is a loxodrome of the sphere : $\frac{Vp}{pp_1}=\frac{Vp_1}{p_1p_2}=$

* See the reports on the instrument published in the *Annales Hydrographiques* and the *Revue Coloniale*, of which what follows is a summary.

$\frac{Vp2}{p2p3} = k =$ constant (as long as the pivot does not alter in shape). In this expression $k = \cdot 5 + E$, E being proportional to $\frac{Cr}{d}$, where C = constant dependent upon the state of surfaces, r = radius of the spherical point, d = distance of the point from the centre of gravity (fig. 5).

The greater the radius of curvature of the point the greater k, the greater the speed with which the axis becomes vertical; k is therefore a coefficient of tendency to verticality. With a mathematically pointed pivot $k = \cdot 5$ and the loxodrome becomes a circumference. All is therefore as if the positions Op, $Op1$, $Op2$ . . . were the directions of a simple pendulum oscillating in the plane OVS.

But $SV = \frac{1}{2}(Sp + Sp1)$ is not accurate; we have

$$SV = Sp + pV = Sp + kpp1,$$
$$SV = Sp1 - Vp1 = Sp1 - kp1p2,$$
$$SV = Sp2 + Vp2 = Sp2 + kp2p3,$$
. . . . . .

Meridian Observation.—If the body is on the meridian three observations are sufficient to find k, for we have from the three equations above $Sp1 - Sp = k(pp1 + p1p2)$, and $k = \frac{pp1}{pp1 + p1p2}$

Let $pp1 = M$, $p1p2 = N$, then $k = \frac{M}{N+M}$, M and N being the amplitudes of the first and second oscillations respectively. For the three positions of the star shown (fig. 3) $k = \frac{139}{139+114} = \cdot 55$.

If Ai is the reading on the limb, $s1$, $s2$ two successive readings on the grating, sx the reading that would have been made if the axis had been vertical, then the calculated altitude is $Ac = Ai + sx$. When the first reading $s1$ was taken S was $s1$ from the central line, which was $pV = kpp1 = Mk$ from the horizon, and so $sx = s1 \pm Mk$, then

$$Ac = Ai + s1 \pm Mk, \text{ and similarly } Ac = Ai + s2 \pm Nk \ldots .$$

But sx is between $s1$ and $s2$; no error on the sign of M or N is possible, it is always the opposite sign to $s1$ for M or $s2$ for N.

The reading on the limb Ai has, of course, been corrected for the instrumental error: this is complex, being partly due to want of parallelism between the reflecting mirrors of the sextant, to want of perpendicularity of these with regard to the plane of the instrument, and to the collimation error of the gyroscope itself, the latter error being due to the fact that the centre of the grating is not always exactly in the plane of the sensible horizon, even when the axis is vertical. Another correction in

the form of a multiplying factor to the readings on the grating is necessary, when the space between the lines is not 10′ but, owing to mechanical inaccuracy, more or less. The correcting constants are ascertained either by observing an object of known altitude or by pointing on a collimator.

Observations out of the Meridian.—When the body is out of the meridian, one of the data of the observation is the time corresponding to a given altitude. This is obtained by time observations taken by an assistant at the required moment. If $s1$ and $s2$ are two consecutive readings, $t1$, $t2$ the corresponding times, s'' what the second reading would be if the body were fixed, then $s2 = s'' + dh(t2-t1)$, where dh is the motion in altitude of the body.

$$Ac = Ai + s1 + (s'' + dh(t2-t1) - s1)k$$
$$Ac = Ai + s1 + (s'' - s1)k + dh(t2-t1)k.$$

The calculated altitude is the altitude at time $t1$ plus the motion in altitude during the time $(t2-t1)k$, and the corresponding time is $t1+(t2-t1)k$. We have, therefore, $Ac = Ai + s1 + (s2-s1)k$, $To = t1+(t2-t1)k$. If k is affected by an error dk, the first altitude obtained by $s1$ and $s2$ will be affected by an error $dAi = +M dk$, and the second, given by $s2$ and $s3$, by an error $dAi = -N dk$; the mean altitude will be affected by an error $(M-N)dk$. It is, therefore, an advantage to take three readings, and to take the mean of the two altitudes. The experience shows that dk is generally not much greater than ·02. The error of altitude is then about 1′.

To get the true altitude we have then

$A = Ai + \sigma s_1 + \sigma k(s_2 - s_1) \pm e \pm i$ — refraction $- \frac{1}{2}$ diameter, where

e is the instrumental correction for the sextant and the gyroscope combined ;

i is the correction, to be considered later, of the influence of the motion of the earth.

σ is the correcting factor due to the interline space not being 10′.

The above method is open to the objection that the difference between the approximate altitude Ai and the calculated one Ac is very large in $Ac = Ai + k(s2-s1)$. The calculation of $k = \frac{M}{M+N}$ is also rather long.

A method of calculation due to Commandant Guyou overcomes these defects, and is worth mentioning.

Meridian Observation.—The above process is not rational, as it gives to three readings the weight of which is the same widely different functions. A first simplification is to take as approximate reading the mean of the two first readings, as follows : when $s1$ is taken, the mean line of the grating is distant from the sensible horizon by $pV = pp1k = Mk$; $s1$ referred to the sensible horizon is therefore $s'_x = s1 \pm Mk$, and $s2$ referred to that horizon

is $s''_x = s2 \pm Nk$. The mean will become $s_x = \frac{1}{2}(s'_x + s''_x) = \frac{1}{2}(s1 \pm Mk + s2 \pm Nk) = \frac{1}{2}(s1 - s2) + k\frac{M-N}{2} = \frac{s1+s2}{2} + \frac{M(M-N)}{2(M+N)}$, but $\frac{1}{2}M = \frac{1}{2}pp1 = \frac{1}{2}(Sp1 - Sp2) = \frac{1}{2}(s2 - s1)$, so that

$$s_x = \tfrac{1}{2}(s1+s2) - (M-N)\frac{\frac{1}{2}(s2-s1)}{M+N}.$$

Let $\frac{M-N}{M+N} = R$, then $s_x = \frac{1}{2}(s1+s2) + \frac{1}{2}R(s2-s1)$; the correction is much smaller. The mean of the means is $\frac{1}{2}\left(\frac{s1+s2}{2} + \frac{s2+s3}{2}\right)$; then

$$s_x = \tfrac{1}{2}\left(\frac{s1+s2}{2} + \frac{s2+s3}{2}\right) + \tfrac{1}{2}R\left(\frac{s2-s1}{2} - \frac{s2-s3}{2}\right).$$

The correction is smaller still ; it may be written

$$\frac{M-N}{2(M+N)}\left(\frac{pp1}{2} - \frac{p1p2}{2}\right) = \frac{M-N}{2(M+N)}\left(\tfrac{1}{2}M - \tfrac{1}{2}N\right), \text{ and we have}$$

$$s_x = \tfrac{1}{2}\left(\frac{s1+s2}{2} + \frac{s2+s3}{2}\right) + \frac{(M-N)^2}{4(M+N)}.$$

The correction being very small, a set of curves gives its value with a sufficient exactitude. (See *Annales Hydrographiques* for 1901.)

Observations out of the Meridian.—It is preferable to reduce all the observations to the same instant by correcting for the motion in altitude of the celestial body : $dh = \frac{1}{4}\sin Z \cos L d T$, and the case is simplified into the preceding one, where the body has no motion along its vertical. The calculation is long, but is simplified by two tables due to Enseigne de Vaisseau Cretin (*Annales Hydrographiques*, 1901, pp. 96 and 98). Table I. gives in seconds of arc the motion of a body in altitude for each second of time : $dh = 15 \sin Z \cos L$, argument : Z and L. Table II. gives, in function of the result found in Table I., the movement in altitude for an interval of 30 to 74 seconds, argument : T and dh from Table I.

Influence of the Motion of the Earth.—As may be expected the motion of the gyroscope is perceptibly influenced by the motion of the Earth. The direction of the motion of precession depends on the direction of rotation of the gyroscope and on the position of the pivot with regard to the centre of gravity. The rotation being clockwise, looking from above the instrument, the precession is counter-clockwise.

The effect of the rotation of the Earth is to make the axis of the gyroscope precess round a deviated vertical, the true zenith being then between the apparent zenith and the celestial pole. For our hemisphere the altitudes observed, facing north, will be always too small, those facing south always too large, the sign of the correction depending of course on the direction of rotation

of the gyroscope. At the pole, for $L = 90°$, and when the observed body is due east or west, the correction is zero. It is maximum for meridian observations, being $\sin i = \frac{2T \cos L}{86400}$ (fig. 6). Out of the meridian in S the correction is $ZE = Z'S - ZS = ZZ' \cos Z$ approximately, Z being reckoned from the north pole always. The correction is, therefore, $i' = \frac{2T \cos Z \cos L}{25}$, its sign depending on the sign of Z and L.

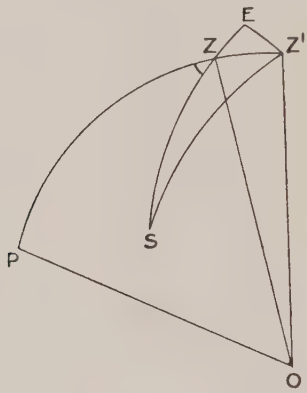


Fig. 6.

The correction being proportional to the time of precession, this time must be known. In the method by maxima and minima it is given by the interval between two consecutive maxima or minima. However, when the method of damped precession is resorted to this time cannot be ascertained by observation. For a small inclination of the axis with regard to the vertical, and for a theoretically sharp pivot, the speed of precession $V = \frac{gd}{r^2 w}$, where g = the gravitation constant, r = the radius of gyration, and w = the angular speed of rotation. If T is the time taken to perform half a turn of precession, $V = \frac{2\pi}{2T} = \frac{gd}{2\pi N r^2}$, where N is the revs. per second and d the distance of the point from the centre of gravity. Then $T = \frac{2\pi^2 r^2 N}{gd} = \frac{2r^2 N}{d}$ nearly, writing $\pi^2 = g$. The time of precession is therefore independent of the mass and inclination of the gyroscope. The correction becomes then, if $c = \frac{4r^2 N}{25d}$,

$$i' = \frac{4r^2 N \cos Z \cos L}{25d} = c \cos Z \cos L.$$

The number of turns can only be ascertained by careful stroboscopic measurements, and this being a delicate operation it would be impossible to perform it as an auxiliary observation when taking the altitudes. A lengthy series of observations was therefore made in 1902 and 1903 by M. L. Favé, at the Service Hydrographique, to ascertain how far the theory was able to give formulæ from which the number of turns could be deduced.

The mechanical constants necessary for these investigations were obtained by oscillating the gyroscope at rest. The gyroscope is really a compound pendulum, and its oscillations are of a somewhat complicated nature, being plane but for a very short time and soon becoming conical. The axis then describes an elliptic cone, the major axis of which rotates in azimuth, the eccentricity decreasing till, after the major axis has turned 45°, the ellipse has become a circle; after this it becomes an ellipse again. After a while the oscillation is again in a plane perpendicular to the primitive plane, and the same series of motions reproduces itself in reverse order till the oscillation is again in the primitive plane. This is due to the fact that the body is not entirely of revolution; moreover the pivot is not theoretically sharp. The determination of the time of oscillation is therefore a delicate operation.

We have then

$$\theta = \pi\sqrt{\frac{d+\frac{B}{md}}{g}}$$

where

θ is the time of a simple oscillation in seconds,

d is the distance of the point of the pivot to the c. g.,

B is the moment of inertia with regard to a perpendicular to the axis of figure passing through the c. g.;

$m = w/g$ is the mass in grams.

Two values of θ were sufficient to obtain B and d. From B the value of the radius of gyration r was obtained; the value of c could therefore be calculated for any value of N.

Measurements were also taken with a stroboscope at different times, so as to give the decrease of N corresponding to the time elapsed since the launching of the gyroscope. This was done for various degrees of vacuum, and a set of curves was obtained (see *Annales Hydrographiques* for 1904). The time of half a cycle of precession being ascertained at the beginning, when the precession can be easily observed, the time elapsed between this observation and the astronomical observation gives, on the curve corresponding to the vacuum indicated by the gauge on the instrument, the number of turns at the time the altitude was observed, or, by means of a special scale, c directly.

It was found, however, that, if the stroboscope is not used, the approximation for the correction of the motion of the Earth is but 20 per cent., or about 1′ in the most unfavourable case.

The next improvement needed seems, therefore, a method to measure N at the time of the observation.

The Tables I. and II. mentioned above may also be used for the correction of the Earth's motion. For $i' = \frac{2T \cos Z \cos L}{25}$, $i' = \frac{T}{3} \frac{15 \cos Z \cos L}{60}$, nearly. Table I. gives $15 \sin Z \cos L$; if it is entered with $90° - Z$ it gives $15 \cos Z \cos L$. Table II. gives the product of T by this number from Table I. reduced to minutes—that is, $\frac{15T \cos Z \cos L}{60}$. Table II. gives therefore a number equal to three times the correction.

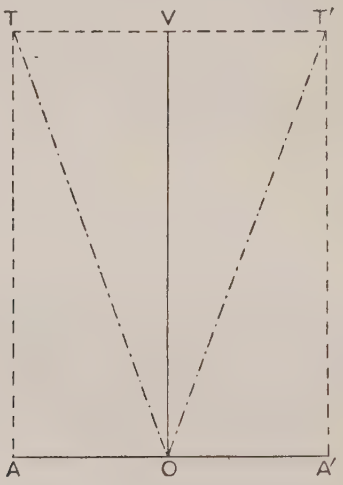


Fig. 7.

Influence of the Motion of the Ship.—The motion of the ship communicates to the pivot O horizontal accelerations OA, OA′; OV being the vertical, the axis precesses around it. If gravitation ceased to act it would precess around OA. The motion of the ship makes it to precess around the resultant OT. The pole will oscillate from T to T′ according to the direction of acceleration (fig. 7).

The rolling motion from one extreme position on one side to that on the other side, if the total time of a single oscillation be t, consists in three phases of approximately equal duration $t/3$, the first being one of uniformly accelerated motion; the second of uniform motion, the last of uniformly retarded motion.

If the motion of the ship was uniform the radius Vp would describe around the vertical 3° in 1 sec. about, if 2T is equal to two minutes. The rolling of the ship will bring the pole successively in T, V, T′ . . . V being a point on the true vertical and T, T′ points on the deviated vertical (fig. 8).

Suppose a rolling period of six secs. for a single oscillation, let the acceleration be in the direction VT, the pole will precess round T for two seconds and will reach 1, the next two seconds (period of uniform motion) it will precess round V and reach 2, then round T′ and reach 3 . . &c.

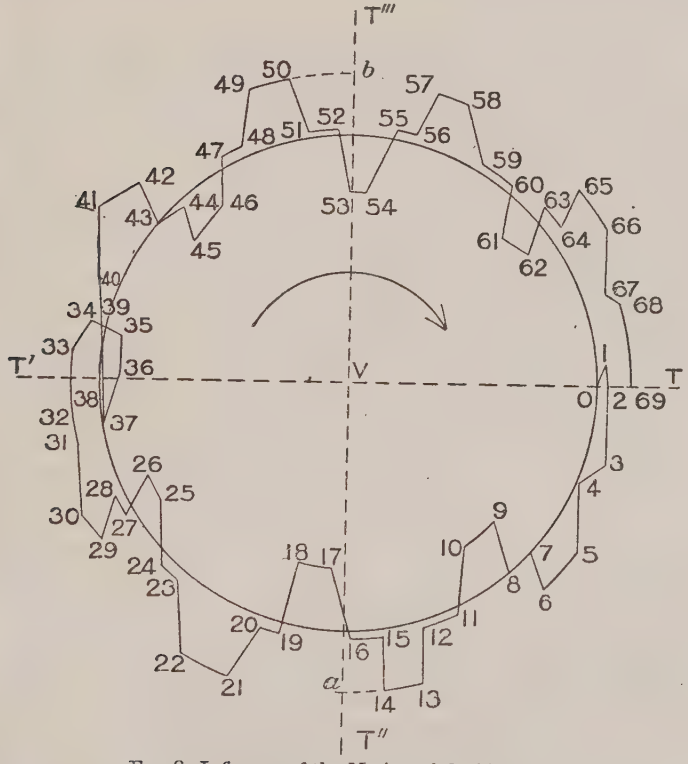


Fig. 8. Influence of the Motion of the Vessel.
(Precession 2^m 18^s, clockwise.)

We see that in the direction TT′ the path of the pole differs little from the circle it would follow if the ship were steady. In the direction T″T‴ the deviation is considerable. In this direction, if the ship were motionless, the central line of the image of the grating would rise above the sensible horizon and descend below it in a quantity corresponding to VO. When the ship is

rolling it will ascend in the field to, say, the position 50, stop from 53 to 54, and rise again till 57, after which the descent will be more marked. After *b* the same thing will occur and the centre of the image of the grating will attain its lower position at 13; V_1a, V_2b are about equal, the middle of the maximum angle coincides approximately with the trace of the sensible horizon. If one observes in the direction perpendicular to the swell—that is, in the direction of the acceleration, as on the beam of a rolling ship—the error will be very small. In the perpendicular direction only the extreme positions on the grating must be observed, and one must be careful not to observe false maxima or minima.

In practice Messrs. Arago, Baule, Boyer, and Schwerer have found that in small boats the error is always negligible, in ships at anchor it is negligible if the extreme positions are observed, on a ship moving with moderate speed in moderate weather the maximum error is less than 3′, and on a fast ship in a heavy sea (this practically never occurring in a fog) the error may be 5′ and more.

This drawback will perhaps be considered serious, but it detracts but little from the utility of the instrument, for the occasions when in heavy weather the sea horizon is not visible are exceedingly rare. At night, if the stars are out, they may be selected in a favourable direction, and even if this cannot be done the course of the ship can be altered for few minutes while the observation is taken, as is often done with far less necessity to ascertain the deviation of the compass on a course different to that which the ship is following.

The gyroscopic collimator (or gyroscopic horizon) of Admiral Fleuriais seems, so far, to be the instrument which, at sea, is likely to give results that, while they are at least as accurate as (and, if the method of a series of observations in rapid succession is resorted to, much more accurate than) those obtained with any other artificial horizon, are obtained in such an easy and convenient way that it cannot be compared with any other instrument used for the same purpose. All the necessary preparations can be made under shelter, in the chart room, and on the observer setting his foot on deck the instrument is at once ready for observing. Any one who has tried to take observations with an artificial horizon in actual sea-going weather and conditions, and not merely to kill time on a fine sunny day, will welcome the instrument as a great step towards the possibility of observing in any weather when there is "something out" to observe.

A certain skill is needed to obtain good results, and care must be taken in handling the instrument, but a man in command of a large modern steamer is not likely to lack in the necessary ability.

The cost of the complete apparatus, octant, gyroscope, pump, accumulators, and spare pieces, is 26*l*. The total weight of the instrument is $4\frac{1}{2}$ lbs.

Cometary Observations at the Liverpool Observatory, 1902–3. By W. E. Plummer.

Comet 1903 *II.* (1902 *d*), *Giacobini.*

Greenwich Mean Time of Observation 1902.		☄ − ✱ R.A.	No. of Comp.	Apparent R A. of ☄	☄ − ✱ Decl.	No. of Comp.	App. Decl. of ☄	Log. Factor of Parallax in α.	in δ.	Star of Comp.
1902	h m s	m s		h m s	′ °		° ′ ″			
Dec. 3	12 10 6·2	−1 20·38	Ret.	7 17 28·19	+ 1 24·7	Ret.	− 1 51 47·0	−0·4801	0·8564	1
6	11 43 16·0	+ 54·33	...	7 16 36·35	− 1 31·0	...	− 1 26 32·9	−0·5161	0·8543	2
11	12 22 29·4	−1 6·31	...	7 14 44·97	+ 1 51·1	...	− 0 38 14·6	−0·3188	0·8513	3
11	12 22 29·4	+1 44·01	...	7 14 44·83	− 2 45·4	...	− 0 38 15·5	−0·3188	0·8513	4
23	9 40 22·9	− 19·74	...	7 8 32·20	+ 6 0·3	...	+ 1 46 13·9	−0·6097	0·8396	5
1903										
Jan. 16	8 40 5·2	− 34·35	...	6 51 37·31	− 16·5	...	+ 8 34 6·0	−0·5139	0·8028	6
21	10 2 5·0	− 55·25	...	6 48 7·57	+ 3 33·2	...	+10 13 24·4	−0·0253	0·7796	7
23	9 21 6·3	+ 14·49	...	6 46 50·64	− 2 1·3	...	+10 52 38·3	−0·2510	0·7771	8
Feb. 17	9 0 13·2	+ 9·69	...	6 36 44·40	+ 6 48·8	...	+18 59 21·9	+9·3472	0·6948	9
20	9 11 16·5	+ 55·28	...	6 36 31·62	− 9 56·6	...	+19 53 17·2	+9·9086	0·6861	10

Comet 1903 *I.* (1903 *a*), *Giacobini.*

Jan. 21	6 33 14·0	+ 44·80	10	22 59 52·57	− 1 36·5	5	+ 2 44 45·5	0·6070	0·8385	11
23	6 35 40·3	− 39·96	10	23 2 2·94	+ 0 20·8	5	+ 3 13 44·6	0·6191	0·8360	12
28	6 10 59·7	+1 34·40	10	23 7 52·42	+ 1 53·1	5	+ 4 30 34·6	0·6029	0·8298	13
28	6 10 59·7	− 10·74	10	23 7 52·37	− 5 35·9	5	+ 4 30 35·3	0·6029	0·8298	14
29	6 20 26·2	+ 11·99	Ret.	23 9 7·74	+ 0 57·7	Ret.	+ 4 46 55·0	0·6274	0·8303	15

Greenwich Mean Time of Observation 1903.				✐ − ✱ R.A.	No. of Comp.	Apparent R.A. of ✐			✐ − ✱ Decl.	No. of Comp.	App. Decl. of ✐			Log. Factor of Parallax in α.	in δ.	Star of Comp.
1903		h	m	s	m s		h	m	s	′ °		°	′	″			
Feb.	3	6	32	19·1	+ 21·35	10	23	15	44·61	− 7	49·9	5	+ 6	12	38·3	0·6556	0·8267	16
	12	6	30	43·0	−2 3·50	10	23	29	19·25	− 3	25·5	5	+ 9	5	37·6	0·6775	0·8184	17
	17	6	39	36·5	− 39·19	12	23	37	51·92	−11	38·8	4	+10	52	12·5	0·6989	0·8169	18
	17	6	41	29·8	+ 18·72	12	23	37	52·04	+ 9	53·0	4	+10	52	14·8	0·7004	0·8172	19
	24	6	40	52·9	+2 14·39	10	23	51	1·30	+ 5	3·2	5	+13	29	57·3	0·7137	0·8120	20
	24	6	40	52·9	+1 41·54	10	23	51	1·27	− 7	30·8	5	+13	29	54·0	0·7137	0·8120	21
Mar.	6	6	41	16·0	+ 15·40	10	0	11	14·51	− 9	56·2	5	+16	51	32·5	0·7321	0·8054	22
	7	6	50	3·2	− 53·89	10	0	13	11·74	+ 2	27·7	5	+17	4	29·6	0·7363	0·8130	23
	10	6	50	0·4	−3 21·51	10	0	18	35·73	+ 2	41·0	5	+17	24	40·0	0·7387	0·8147	24
	11	6	52	5·6	−1 45·26	10	0	20	11·99	+ 1	25·1	5	+17	23	23·9	0·7390	0·8155	24
	12	6	51	53·0	−1 18·85	10	0	21	40·42	− 4	20·7	5	+17	16	59·3	0·7393	0·8168	25

Comet 1903 *IV.* (1903 *c*), *Barrelly.*

June	22	12	43	1·1	+ 45·90	12	21	51	55·20	+ 7	33·7	6	− 7	18	34·2	−0·6114	0·8698	26
	28	12	2	35·3	− 5·57	10	21	45	12·49	+ 7	42·2	4	− 0	55	28·7	−0·6232	0·8512	27
	28	12	2	35·3	+2 54·20	10	21	45	12·39	+ 2	48·2	4	− 0	55	28·4	−0·6232	0·8512	28
	29	11	47	22·9	+1 33·21	8]	21	43	32·27	+ 3	41·5	4	+ 0	28	12·2	−0·6364	0·8575	29
July	1	11	48	26·0	−4 1·13	10	21	35	25·99	− 5	31·4	5	+ 3	41	36·7	−0·6130	0·8338	30
	7	11	10	46·9	−2 22·22	10	21	18	19·09	+ 2	8·5	5	+18	5	2·3	−0·6294	0·7524	31
	7	11	10	46·9	− 6·04	10	21	18	18·87	− 7	28·0	5	+18	5	6·9	−0·6294	0·7524	32

Greenwich Mean Time of Observation 1903.				☄ − ✱ R.A.	No. of Comp.	Apparent R.A. of ☄			☄ − ✱ Decl.	No. of Comp.	App. Decl. of ☄			Log. Factor of Parallax in α.	in δ.	Star of Comp.
1903	h	m	s	m s		h	m	s	′ ″		°	′	″			
July 10	10	40	16·3	− 10·69	8	20	58	15·93	+ 2 22·3	4	+29	9	18·0	−0·6642	0·6534	33
10	10	40	16·3	+ 35·63	8	20	58	15·96	− 5 50·1	4	+29	9	18·4	−0·6642	0·6534	34
13	10	30	22·5	−1 25·65	10	20	23	31·32	+ 3 55·1	5	+43	9	12·8	−0·6733	0·3822	35
15	9	40	14·2	−1 37·32	8	19	45	10·23	+10 22·3	4	+52	58	55·7	−0·7655	0·0432	36
16	9	32	11·0	−1 46·15	10	19	17	56·67	+ 4 25·2	5	+57	39	26·2	−0·7624	9·2314	37
22	10	5	20·8	−9 1·83	10	14	37	39·60	− 4 4·0	5	+67	44	59·9	1·0170	−9·8115	38
24	9	42	16·0	+3 40·88	8	13	30	17·97	− 2 20·7	4	+64	38	42·0	1·0334	9·8676	39
26	9	40	46·4	+1 13·14	10	12	47	8·17	− 24·1	5	+60	57	12·0	1·0177	0·3291	40
26	9	40	46·4	+2 42·90	10	12	47	8·50	+ 6 9·5	5	+60	57	13·6	1·0177	0·3291	41
28	9	32	19·0	+2 16·76	12	12	18	54·25	− 5 21·4	4	+57	25	47·2	0·9847	0·4871	42
28	9	31	26·3	−1 30·69	10	12	18	53·97	+ 6 48·4	4	+57	25	49·3	0·9847	0·4871	43
29	9	21	11·2	+5 1·86	8	12	8	17·40	−11 45·4	4	+55	48	32·9	0·9674	0·5207	44
Aug. 3	9	16	15·0	+2 55·35	10	11	32	59·56	+ 55·3	5	+49	0	36·3	0·9039	0·6903	45
4	9	29	20·9	− 19·99	10	11	27	59·62	− 9 11·6	5	+47	52	7·8	0·8897	0·7290	46
4	9	29	20·9	− 2·03	10	11	27	59·54	+ 5 22·1	5	+47	52	7·9	0·8897	0·7290	47
6	9	18	26·3	−3 17·13	12	11	19	15·50	+ 4 1·0	5	+45	47	9·7	0·8689	0·7520	48
9	9	19	59·0	−2 29·21	10	11	7	59·71	− 50·6	5	+42	57	48·9	0·8377	0·7934	49
10	9	24	30·4	+3 49·79	8	11	4	32·96	− 4 14·2	4	+42	4	44·0	0·8239	0·8092	50
12	9	16	28·3	−2 42·64	10	10	58	0·62	+ 3 3·4	5	+40	22	7·4	0·8080	0·8217	51

Stars of Comparison. Comet 1903 *II.*

Date. 1902.	Star's Designation or Authority for Place.	Mean R.A. Equinox of Year. h m s	Correction to Mean R.A. s	Mean Declination Equinox of Year. ° ′ ″	Correction to Mean Declination.	No. of Reference.
Dec. 3	A.G.Z. Nicolajew, No. 2150	7 18 44·33	+4·24	− 1 52 59·3	−12·4	1
6	A.G.Z. „ No. 2132	7 15 37·70	+4·32	− 1 24 49·1	−12·8	2
11	A.G.Z. „ No. 2134	7 15 46·84	+4·44	− 0 39 51·9	−13·8	3
11	Gottingen$_2$, No. 2436–7	7 12 56·37	+4·45	− 0 40 47·2	−13·7	4
23	A.G.Z. Albany No. 2688	7 8 47·19	+4·75	+ 1 40 29·0	−15·7	5
1903. Jan. 16	A.G.Z. Leipzig II., No. 3378	6 52 9·74	+1·92	+ 8 34 35·1	−12·6	6
21	A.G.Z. Leipzig I., No. 2571	6 49 0·89	+1·93	+10 10 3·9	−12·7	7
23	A.G.Z. Leipzig I., No. 2542	6 46 34·21	+1·94	+10 54 52·3	−12·7	8
Feb. 17	A.G.Z. Berlin A., No. 2328	6 36 32·87	+1·84	+18 52 44·4	−11·3	9
20	A.G.Z. Berlin A., No. 2318	6 35 34·54	+1·80	+20 3 24·7	−10·9	10
	Comet 1903 *a.*					
Jan. 21	A.G.Z. Albany, No. 7960	22 59 8·05	−0·28	+ 2 46 19·2	+ 2·8	11
23	A.G.Z. „ No. 7979	23 2 43·18	−0·28	+ 3 13 21·2	+ 2·6	12
28	A.G.Z. „ No. 8002	23 6 18·30	−0·28	+ 4 28 39·1	+ 2·4	13
28	A.G.Z. „ No. 8010	23 8 3·39	−0·28	+ 4 36 8·8	+ 2·4	14
29	A.G.Z. „ No. 8019	23 8 56·03	−0·28	+ 4 45 55·0	+ 2·3	15
Feb. 3	A.G.Z. Leipzig II., No. 11610	23 15 23·53	−0·27	+ 6 20 26·2	+ 2·0	16

Date. 1903.	Star's Designation or Authority for Place.	Mean R.A. Equinox of Year. h m s	Correction to Mean R.A. s	Mean Declination Equinox of Year. ° ′ ″	Correction to Mean Declination.	No. of Reference.
Feb. 12	A.G.Z. Leipzig II., No. 11703	23 31 22·99	−0·24	+ 9 9 1·8	+ 1·3	17
17	Rumker, No. 11503	23 38 31·33	−0·22	+11 3 50·2	+ 1·1	18
17	A.G.Z. Leipzig I., No. 9407	23 37 33·55	−0·23	+10 42 20·7	+ 1·1	19
24	A.G.Z. „ No. 9476	23 48 47·11	−0·20	+13 24 53·5	+ 0·6	20
24	A.G.Z. „ No. 9482	23 49 19·93	−0·20	+13 37 24·2	+ 0·6	21
Mar. 6	A.G.Z. Berlin A, No. 47...	0 10 59·26	−0·15	+17 1 28·9	− 0·2	22
7	A.G.Z. „ No. 65...	0 14 5·78	−0·15	+17 2 2·2	− 0·3	23
10	A.G.Z. „ No. 100	0 21 57·38	−0·14	+17 21 59·6	− 0·6	24
11	„ „ „	...	−0·13	...	− 0·8	24
12	A.G.Z. „ No. 109	0 22 59·40	−0·13	+17 21 20·9	− 0·9	25

Stars of Comparison. Comet 1903 *IV.*

Date. 1903.	Star's Designation or Authority for Place.	Mean R.A. Equinox 1903. h m s	Correction to Mean R.A. s	Mean Declination Equinox, 1903. ° ′ ″	Correction to Mean Declination.	No. of Reference.
June 22	A.G.Z. Ottak., 85, 166	21 51 6·90	+2·40	− 7 26 24·6	+16·7	26
21	A.G.Z. Nicolajew, No. 5517	21 45 15·46	+2·60	− 1 3 26·8	+15·9	27
21	„ „ No. 5512	21 42 15·59	+2·60	− 0 58 32·5	+15·9	28
29	„ „ No. 5510	21 41 56·42	+2·64	+ 0 24 14·8	+15·9	29
July 1	A.G.Z. Albany, No. 7590	21 39 24·41	+2·71	+ 3 46 52·9	+15·2	30
7	A.G.Z. Berlin A, No. 8737	21 20 38·35	+2·96	+18 2 40·3	+13·5	31

Date. 1903.	Star's Designation or Authority for Place.	Mean R.A. Equinox 1903. h m s	Correction to Mean R.A. s	Mean Declination Equinox, 1903. ° ′ ″	Correction to Mean Declination.	No. of Reference.
July 7	A.G.Z. Berlin A., No. 8720	21 18 21·94	+2·97	+18 12 21·4	+13·5	32
10	A.G.Z. Camb., Eng., No. 12034 ...	20 58 23·48	+3·14	+29 6 43·0	+12·7	33
10	„ „ „ No. 12017 ...	20 57 37·18	+3·15	+29 14 55·8	+12·7	34
13	A.G.Z. Bonn, No. 14257	20 24 53·59	+3·38	+43 5 4·8	+12·9	35
15	A.G.Z. Camb., U.S.A., No. 6218 ...	19 46 44·03	+3·52	+52 48 19·2	+14·2	36
16	A.G.Z. Hels. No. 10424...	19 19 39·27	+3·55	+57 34 45·6	+15·4	37
22	A.G.Z. Christiania, No. 2207	14 46 41·10	+0·33	+67 48 47·7	+16·2	38
24	A.G.Z. Hels. No. 7596 & Chris. No. 2013	13 26 37·40	−0·31	+64 40 51·3	+11·4	39
26	A.G.Z. Hels. Gotha, No. 7329	12 45 55·34	−0·31	+60 57 28·3	+ 7·8	40
26	„ „ „ No. 7318	12 44 25·91	−0·31	+60 50 56·5	+ 7·6	41
28	„ „ „ No. 7120	12 16 37·75	−0·24	+57 31 3·7	+ 4·9	42
28	„ „ „ No. 7156	12 20 24·87	−0·21	+57 18 55·7	+ 5·2	43
29	„ „ „ No. 7030	12 3 15·74	−0·20	+56 0 14·8	+ 3·5	44
Aug. 3	„ Bonn, No. 8110	11 30 4·15	+0·06	+48 59 41·6	− 0·6	45
4	„ „ No. 8099	11 28 19·52	+0·09	+48 1 20·4	− 1·0	46
4	„ „ No. 8072	11 28 1·47	+0·10	+47 46 47·1	− 1·3	47
6	„ „ No. 8036	11 22 32·46	+0·17	+45 43 10·7	− 2·0	48
9	„ „ No. 7956	11 10 28·67	+0·25	+42 58 43·1	− 3·6	49
10	Differenced from Bonn, No. 7966 ...	11 11 32·19	+0·27	+42 15 7·9	− 3·7	50
12	A.G.Z. Bonn, No. 7891	11 0 42·93	+0·33	+40 19 9·0	− 5·0	51

Results of Micrometric Measures of Double Stars made with the 28-inch Refractor at the Royal Observatory, Greenwich, in the year 1903.

(*Communicated by the Astronomer-Royal.*)

The measures were made with a bifilar position micrometer on the 28-inch refractor, focal length 28 feet. The power generally used was 670, but when the definition permitted a power of 1120 was employed for observing very close pairs. A blue glass shade was employed to diminish the light and irradiation when bright stars were observed. The observations were made in variously coloured fields or in a dark field with illuminated wires. The initials in the last column are those of the observers, viz. :—

L. Mr. Lewis. W. B. Mr. Bowyer.
B. Mr. Bryant. H. F. Mr. Furner.

Star's Name.		R.A. 1900. h m	N.P.D. 1900. ° ′	Position Angle. °	Distance. ″	No. of Nights.	Mags.		Epoch 1903.	Obs.
Σ 3061 ...	...	0 1	72 43	148·4	7·90	1	8·0	8·0	·854	L.
Σ 3060	...	0 1	72 28	121·0	3·47	1	8·5	8·7	·854	L.
Ho. 1. ...	...	0 6	60 57	170·6	1·14	1	8·5	8·8	·991	L.
OΣ 2	A.C.	0 8	63 34	225·1	17·42	1	6·5	9·6	·917	L.
	A.B.			43·2	0·48	3	6·5	8·0	·901	W.B.
				43·6	0·76	1		...	·923	B.
Σ 19 ...	...	0 12	53 55	135·0	2·29	1	7·0	9·5	·824	L.
B.D. + 20°, 18...		0 12	68 47	135·8	1·24	1	8·9	9·4	·991	L.
Σ 24 ...	...	0 13	64 23	248·4	4·94	1	7·2	8·0	·879	B.
				247·2	4·94	1		...	·895	W.B.
Σ 25	...	0 14	74 34	192·0	1·31	1	8·5	8·5	·854	L.
β 779 ...	...	0 23	66 58	253·8	0·84	1	8·9	9·0	·991	L.
OΣ 12 ...	...	0 26	36 2	156·4	0·41	1	5·6	5·9	·802	B.
				160·3	0·39	1		...	·953	W.B.
Σ 46 ...	...	0 35	69 7	191·7	6·81	1	8·0	8·2	·895	W.B.
β 257 ...	...	0 35	43 17	241·9	0·54	1	8·1	8·5	·824	L.
Σ 60 η Cass.	...	0 43	32 43	227·4	5·63	1	4·0	7·6	·953	W.B.
β 495 ...	...	0 43	71 51	226·1	0·60	3	7·5	7·7	·838	W.B.
				218·3	0·71	2		...	·896	H.F.
				224·0	0·76	1		...	·923	B.
				219·6	0·59	1		...	·931	L.
OΣ 20...	...	0 49	71 22	321·7	0·48	1	5·9	7·0	·923	B.
				314·6	0·37	1		...	·931	L.

Star's Name.	R.A. 1900. h m	N.P.D. 1900. ° ′	Position Angle. °	Distance. ″	No. of Nights.	Mags.	Epoch 1903.	Obs.
Σ 73	0 50	66 55	27·6	0·94	1	6·2 6·8	·017	L.
			24·1	1·08	5	...	·699	W.B.
			22·5	1·05	2	...	·796	H.F.
β 1099 ...	0 51	30 11	325·8	0·19	1	6·1 6·5	·802	B.
β 302	0 53	69 9	103·0	0·58	3	6·7 8·0	·580	W.B.
			106·8	0·77	1	...	·849	H.F.
			102·0	0·61	1	...	·923	B.
β 1228 ...	1 1	73 13	285·4	0·80	1	8·3 8·9	·030	W.B.
β 235	1 5	39 30	92·6	0·87	1	7·2 7·3	·953	W.B.
Σ 113	1 15	91 2	357·5	1·54	3	6·2 7·2	·873	H.F.
β 4	1 18	79 9	65·3	0·29	2	7·8 8·8	·855	W.B.
			72·6	0·39	1	...	·923	B.
β 1164 ...	1 22	85 10	174·1	0·58	1	6·7 7·0	·923	B.
Σ 138	1 31	82 52	41·7	1·45	3	7·3 7·3	·599	W.B.
			40·5	1·35	2	...	·895	H.F.
			40·2	1·38	1	...	·879	B.
β 509	1 38	80 56	247·7	0·83	1	8·4 8·7	·923	B.
			253·6	0·80	1	...	·931	L.
Σ 149	1 39	50 32	97·7	1·41	1	8·2 9·7	·808	L.
β 510	1 43	74 10	327·3	1·51	1	8·0 10·5	·931	L.
Ho. 311 ...	1 46	65 50	173·2	0·39	3	7·5 7·5	·082	W.B.
β 260	1 48	75 3	238·4	0·65	3	8·0 8·1	·479	W.B.
			239·0	0·74	1	...	·923	B.
			237·8	0·79	1	...	·931	L.
Σ 183. ... A.B.	1 49	61 41	180·8	0·37	1	7·5 8·2	·499	W.B.
$\frac{A.B.}{2}$ C			162·7	5·32	4	7·5 8·9	·689	W.B.
Σ 186	1 51	88 39	212·3	0·55	1	7·2 7·2	·923	B.
			211·3	0·52	1	...	·931	L.
Σ 205 γ^1 Androm.	1 58	48 10	61·8	10·20	1	3·0 5·0	·808	L.
OΣ 38 γ^2 Androm.	1 58	48 10	116·1	0·43	1	5·0 6·2	·802	B.
			113·8	0·45	1	...	·808	L.
Σ 208	1 58	64 33	73·0	0·63	1	6·2 8·4	·868	W.B.
Σ 228	2 8	42 59	32·0	0·63	1	6·7 7·6	·802	B.
OΣ 44	2 36	47 44	56·2	1·34	1	7·8 8·5	·129	W.B.
β 262	2 42	49 22	72·3	1·59	1	8·2 9·1	·030	W.B.
Σ 305	2 42	71 2	315·3	3·19	2	7·3 8·2	·474	L.
			313·9	3·11	5	...	·398	W.B.
			315·9	2·77	2	...	·497	H.F.

Star's Name.	R.A. 1900.	N.P.D. 1900.	Position Angle.	Distance.	No. of Nights.	Mags.	Epoch 1903.	Obs.
	h m	° ′	°	″				
β 524 A.B.	2 47	52 5	201·9	0·18	1	5·5 6·4	·192	L.
			180·5	0·17	1	...	·802	B.
			219·9	0·17	1	...	·923	B.
$\frac{A.B.}{2}$ C.	...	...	236·8	14·43	2	5·5 9·2	·126	L.
			234·7	13·86	1	...	·088	W.B.
β 1173 ...	2 53	66 16	332·0	0·09	2	7·7 8·8	·038	L.
			339·3	0·20	2	...	·109	W.B.
β 525	2 53	68 47	129·0	0·28	3	7·5 7·5	·336	L.
			128·3	0·17	1	...	·129	W.B.
			132·0	0·25	1	...	·923	B.
Σ 333	2 53	69 4	201·6	1·38	2	5·7 6·0	·474	L.
			202·9	1·27	2	...	·470	W.B.
			201·5	1·19	1	...	·099	H.F.
Σ 346...A.B.	3 0	65 8	267·1	0·43	1	6·0 6·0	·060	L.
			269·4	0·48	3	...	·624	W.B.
$\frac{A.B.}{2}$ C.	...	...	352·0	5·19	1	6·0 10·8	·060	L.
			352·6	5·02	1	...	·088	W.B.
β 530	3 9	67 25	194·3	1·58	1	9·7 10·1	·041	W.B.
OΣ 53	3 11	51 44	228·4	0·65	1	7·2 8·0	·192	L.
			230·8	0·52	1	...	·934	B.
			229·3	0·66	2	...	·938	W.B.
Σ 412...A.B.	3 29	65 52	4·6	0·25	2	6·6 6·7	·471	W.B.
			6·1	...	1	...	·072	L.
			18·6	0·21	1	...	·923	B.
β 533	3 29	58 39	47·2	0·51	4	8·5 8·5	·692	W.B.
			48·7	0·55	1	...	·934	B.
Σ 425	3 34	56 10	90·6	2·44	5	7·3 7·3	·748	W.B.
β 535	3 38	58 2	51·6	0·81	1	4·0 8·5	·923	B.
β 880	3 38	58 9	355·1	0·71	1	8·5 8·5	·923	B.
Hussey 103 ...	3 38	40 27	209·5	0·89	1	8·1 8·4	·115	L.
β 536 $\frac{A.B.}{2}$ C.	3 40	66 6	292·9	38·30	1	8·2 8·2	·060	L.
A.B.	...	...	292·9	...	1	8·2 8·7	·060	L.*
β 1184... ...	3 42	67 56	274·1	0·50	4	8·1 8·3	·472	W.B.
OΣ 66	3 45	49 30	139·6	0·72	1	7·5 8·0	·192	L.
Σ 483	3 57	50 45	229·8	0·46	1	8·0 9·5	·129	W.B.

* Elongated.

Star's Name.	R.A. 1900. h m	N.P.D. 1900. ° ′	Position Angle. °	Distance. ″	No. of Nights.	Mags.		Epoch 1903.	Obs.
OΣ 531 ...	4 1	52 12	129·3	1·90	1	6·5	8·2	·564	W.B.
β 1232 ...	4 3	61 5	344·8	0·50	1	8·4	9·3	·038	L.
Ho. 326 ...	4 3	61 37	165·5	0·33	1	8·5	8·5	·038	L.
β 546	4 4	48 24	32·5	0·92	1	8·2	8·3	·230	L.
			30·8	0·83	1	...		·936	W.B.
OΣ 77	4 10	58 33	195·4	0·23	2	7·0	7·5	·077	W.B.
Σ 535	4 18	78 51	326·3	1·84	3	6·7	8·2	·647	W.B.
			321·4	1·50	1	...		·099	H.F.
β 1235 ...	4 18	67 29	57·4	0·20	1	8·4	8·5	·115	L.
			65·0	0·21	1	...		·194	W.B.
W.B. (2) IV. 583	4 30	70 15	192·4	0·31	3	7·5	8·0	·131	W.B.
OΣ 86	4 31	70 27	65·1	0·51	1	7·5	7·5	·016	L.
			62·5	0·43	3	...		·131	W.B.
Σ 567	4 31	70 43	320·5	1·85	1	8·5	9·0	·016	L.
			318·4	1·87	2	...		·485	W.B.
			320·2	1·70	1	...		·099	H.F.
Σ 572	4 32	63 18	199·1	3·62	1	6·5	6·5	·016	L.
			201·3	3·55	3	...		·352	W.B.
			200·5	3·54	1	...		·099	H.F.
β 883...A.B.	4 46	79 6	91·6	0·23	3	7·5	7·8	·045	L.
β 883...A.C.			157·6	17·60	1	7·5	13·0	·060	L.
OΣ 92 ...	4 53	50 45	253·0	2·99	1	6·0	9·7	·230	L.
OΣ 93 ...	4 55	85 3	43·4	0·64	1	7·5	9·0	·994	W.B.
OΣ 98 ...	5 2	81 38	168·9	0·69	2	6·0	6·8	·518	W.B.
			173·5	0·89	3	...		·131	H.F.
Σ 645	5 3	62 5	25·1	11·78	1	7·2	8·8	·194	W.B.
Σ 687...A.B.	5 16	56 18	68·1	17·36	2	8·1	8·6	·055	L.
β 886...C.D.			253·4	0·85	2	9·1	9·6	·055	L.
OΣ 105 ...	5 16	77 26	286·5	0·53	2	7·8	7·8	·482	W.B.
β 887 A.B.	5 16	56 41	186·5	1·06	1	9·0	10·5	·071	L.
A.C.	...	...	111·0	9·13	1	9·0	13·5	·071	L.
A.D.	...	...	336·7	10·67	1	9·0	12·2	·071	L.
A.E.	...	...	198·6	14·63	1	9·0	13·5	·071	L.
A.F.	...	...	351·6	38·18	1	9·0	12·5	·071	L.
F.f.	...	...	359·5	2·33	1	12·5	13·5	·071	L.
Σ 719 A.B.	5 24	60 32	327·8	1·18	2	7·0	7·0	·055	L.
			330·1	1·16	2	...		·534	W.B.
A.C.	...	...	351·3	15·29	2	7·0	8·9	·055	L.

Star's Name.	R.A. 1900. h m	N.P.D. 1900. ° ′	Position Angle. °	Distance. ″	No. of Nights.	Mags.	Epoch 1903.	Obs.
Σ 749	5 31	63 8	169·4	0·96	2	7·1 7·2	·027	L.
			172·1	0·83	2	...	·086	W.B.
			170·4	0·83	1	...	·118	H.F.
β 1240...A.B.	5 32	59 34	319·1	0·20	2	5·6 6·0	·140	B.
			332·5	0·21	1	...	·129	W.B.
Σ 753...A.C.	...	...	267·6	12·42	2	5·6 8·7	·567	W.B.
OΣ 112 ...	5 33	52 6	69·6	0·60	2	7·5 7·5	·162	W.B.
β 1032 ...	5 34	92 39	337·3	0·23	2	4·0 5·0	·140	B.
β 1007 ...	5 36	73 31	253·7	0·21	2	6·0 6·2	·140	B.
OΣ 118 ...	5 42	69 10	311·6	0·64	4	6·2 7·7	·114	W.B.
			308·7	0·74	1	... ...	·118	H.F.
OΣ 119 ...	5 43	82 4	318·3	0·45	1	7·5 8·3	·189	W.B.
β 560	5 43	60 18	156·7	0·73	1	8·0 8·5	·016	L.
* near β 560 ...	5 43	60 20	165·0	3·93	2	8·8 9·8	·188	L.
Σ 799	5 45	51 28	184·1	1·02	2	7·2 8·3	·162	W.B.
Aitken 54 ...	6 3	60 45	351·7	0·46	1	7·6 8·5	·189	W.B.
β 1241 A.B.	6 4	66 52	339·0	...	1	5·9 10·0	·208	L.
A.C.	...		63·1	17·85	1	5·9 14·0	·208	L.
β 1058 ...	6 4	66 59	287·5	0·32	1	6·4 6·5	·167	B.
			273·7	0·29	1	...	·189	W.B.
Aitken 55 ...	6 5	61 13	288·1	0·58	1	8·7 9·3	·189	W.B.
Aitken 56 ...	6 7	60 56	51·8	1·46	1	8·1 11·8	·189	W.B.
β 895 A.B.	6 14	61 31	143·4	0·24	1	7·5 7·5	·167	B.
			170·4	0·19	1	...	·189	W.B.
Σ 888 $\frac{\text{A.B.}}{2}$ C.	6 14	61 31	253·0	2·50	1	7·5 9·5	·038	L.
			250·9	2·88	1	...	·189	W.B.
Ho. 25... ...	6 16	64 43	250·9	0·89	1	8·8 9·0	·189	W.B.
β 1020 ...	6 17	61 11	155·9	1·12	1	8·2 10·0	·038	L.
Σ 899	6 17	72 22	19·8	2·29	3	7·0 8·0	·096	W.B.
			19·6	2·19	2	...	·109	H.F.
β 1021 ...	6 25	61 32	90·8	0·77	2	8·0 9·0	·169	L.
			83·7	0·74	1	...	·934	B.
Σ 932	6 29	75 10	326·6	2·07	3	8·2 8·3	·094	W.B.
			331·3	1·94	2	...	·109	H.F.
OΣ 149 ...	6 30	62 38	279·3	0·94	1	6·5 9·0	·110	W.B.
			276·9	0·64	2	...	·169	L.
			274·6	0·73	1	...	·923	B.
Σ 945	6 33	48 56	276·2	0·89	1	7·1 8·0	·934	B.

Star's Name.		R.A. 1900. h m	N.P.D. 1900. ° ′	Position Angle. °	Distance. ″	No. of Nights.	Mags.	Epoch 1903.	Obs.
Σ 948	A.B.	6 37	30 28	117·4	1·63	1	5·2 6·1	·934	B.
	A.C.	...		307·1	8·24	1	5·2 7·0	·934	B.
OΣ 156	...	6 42	71 42	298·0	0·48	3	6·5 7·0	·107	W.B.
				299·0	0·89	1	...	·118	H.F.
				300·4	0·54	1	...	·167	B.
OΣ 160	...	6 48	68 43	171·9	1·42	3	6·8 9·8	·101	W.B.
				178·0	1·26	1	...	·145	L.
β 899	A.B.	6 53	71 9	271·8	0·47	1	8·7 9·3	·170	W.B.
	A.C.			175·4	24·01	1	8·7 9·5	·170	W.B.
	A.D.			47·6	40·80	1	8·7 8·9	·170	W.B.
β 327 ...	...	6 54	92 50	100·0	0·64	2	7·8 7·8	·140	B.
Σ 1037	...	7 7	62 36	292·5	0·83	4	7·1 7·1	·109	W.B.
OΣ 170	...	7 12	80 32	108·0	1·65	1	7·5 7·5	·131	W.B.
				107·9	1·30	2	...	·167	H.F.
				107·0	1·19	1	...	·230	L.
Σ 1066	...	7 14	67 50	205·4	6·79	1	3·2 8·2	·131	W.B.
				209·2	6·89	1	...	·175	H.F.
β 21 ...	...	7 23	82 51	25·9	4·23	1	5·7 11·2	·175	H.F.
Σ 1110 (Castor)		7 27	57 54	323·2	5·59	1	2·7 3·7	·153	W.B.
OΣ 175	...	7 29	58 49	330·1	0·55	3	6·0 6·6	·163	W.B.
				331·6	0·65	2	...	·228	B.
Σ 1126	...	7 35	84 32	143·9	1·35	3	7·2 7·5	·140	W.B.
				141·2	1·17	1	...	·175	H.F.
OΣ 177	...	7 35	52 20	115·9	0·47	1	7·5 8·5	·288	B.
β 101 ...	...	7 47	103 38	305·5	0·44	1	5·2 6·5	·167	B.
OΣ 182	...	7 47	86 22	28·4	1·07	1	7·0 7·5	·159	H.F.
OΣ 185	...	7 52	88 36	18·7	0·34	2	6·8 7·0	·140	B.
Σ 1175	...	7 57	85 34	224·8	1·76	1	7·8 9·7	·159	H.F.
β 581	A.B.	7 59	77 26	296·7	0·35	2	8·5 8·6	·140	B.
				287·0	0·54	1	...	·145	L.
				304·7	0·39	1	...	·178	W.B.
	A.C.	...	...	193·3	4·44	1	8·5 10·5	·145	L.
Σ 1187	...	8 3	57 28	44·3	2·42	1	7·1 8·0	·153	W.B.
				46·2	1·93	1	...	·214	H.F.
Σ 1196 (ζ Cancri)	A.B.	8 6	72 3	357·0	1·09	2	5·0 5·7	·167	H.F.
				357·6	1·01	1	...	·208	L.
				356·4	1·07	4	...	·214	W.B.
	A.C.	...	...	111·5	5·36	1	5·0 6·5	·175	H.F.

Star's Name.		R.A. 1900. h m	N.P.D. 1900. ° ′	Position Angle. °	Distance. ″	No. of Nights.	Mags.	Epoch 1903.	Obs.
				110·6	5·38	1	...	·208	L.
				109·1	5·25	2	...	·211	W.B.
	B.C.	...	...	126·0	5·86	2	5·7 6·5	·256	W.B.
				124·8	6·03	1	...	·175	H.F.
				123·4	5·99	1	...	·208	L.
β 1244	...	8 7	89 43	37·4	0·69	1	7·9 8·1	·167	B.
β 1243	...	8 8	72 2	346·5	1·03	1	7·1 13·0	·208	L.
Σ 1202	...	8 8	78 51	317·0	2·59	1	7·7 9·8	·145	L.
				314·7	2·58	1	...	·170	W.B.
				322·0	2·41	1	...	·175	H.F.
Σ 1216	...	8 16	91 17	199·0	0·37	1	7·5 8·2	·167	B.
β 585 ...	...	8 36	69 10	106·4	0·59	1	7·7 9·0	·170	W.B.
				108·4	0·49	1	...	·208	L.
Σ 1273	A.B.	8 41	83 13	26·6	0·34	1	3·8 6·0	·282	W.B.
	$\frac{A.B.}{2}$ C.	...	...	230·3	3·44	1	3·8 3 7	·145	L.
				235·8	3·34	3	...	·246	W.B.
				235·1	2·90	2	...	·225	H.F.
β 1068	A.B.	8 44	80 46	189·0	0·30	2	7·7 8·8	·236	W.B.
	A.C.	...	...	310·9	17·58	1	7·7 13·0	·282	W.B.
Perrotin	...	8 46	81 17	347·7	0·69	1	7·5 8·7	·145	L.
				350·2	0·65	2	...	·249	W.B.
Σ 3121	...	9 12	61 0	24·8	0·79	3	7·5 7·8	·189	B.
				26·9	0·68	2	...	·184	W.B.
				20·1	0·73	1	...	·230	L.
Aitken 221	...	9 13	59 50	304·3	0·44	1	8·7 8·8	·304	L.
				301·6	0·43	1	...	·304	W.B.
Ho. 43...	...	9 13	68 47	297·5	0·47	2	8·0 8·5	·229	W.B.
				291·2	0·49	1	...	·208	L.
OΣ 201	...	9 18	61 39	221·2	1·53	4	7·5 9·0	·176	W.B
β 1070...	...	9 18	63 20	82·8	0·70	1	9·1 10·2	·309	W.B.
Σ 1348	...	9 19	83 13	319·2	1·73	2	7·5 7·6	·320	W.B.
Aitken...	...	9 20	58 25	43·5	0·84	1	8·7 10·2	·304	L.
				44·2	1·11	1	...	·304	W.B.
Aitken 222	...	9 20	60 55	324·3	0·34	1	8·3 8·5	·309	W.B.
Σ 1356	...	9 23	80 31	111·0	0·92	1	6·2 7·0	·167	B.
				117·6	0·82	2	...	·223	W.B.
				115·6	0·80	1	...	·285	L.
				117·5	0·72	1	...	·290	H.F.

Star's Name.	R.A. 1900.	N.P.D. 1900.	Position Angle.	Distance.	No. of Nights.	Mags.	Epoch 1903.	Obs.
	h m	° ′	°	″				
Σ 1374 ...	9 35	50 35	286·4	2·94	1	7·0 8·3	·230	L.
Σ 1385 ...	9 44	72 58	353·4	1·18	2	8·5 10·7	·247	L.
OΣ 208 ...	9 45	35 28	112·4	0·26	2	5·0 6·0	·249	B.
Σ 1389 ...	9 47	62 32	308·6	2·18	3	8·2 9·0	·155	W.B.
			312·1	1·98	1	...	·236	H.F.
A.C. 5	9 48	97 38	85·5	0·41	1	5·5 5·7	·167	B.
Regulus B.C. ...	10 3	77 33	81·8	2·25	1	8·5 10·0	·318	L.
W.B. (2) X 128·9	10 10	71 38	6·0	1·32	3	8·0 8·5	·181	W.B.
			10·0	1·32	1	...	·285	L.
OΣ 215 ...	10 11	71 46	206·5	0·74	3	7·0 7·2	·181	W.B.
			189·8	1·06	1	...	·167	B.
			208·4	0·92	1	...	·285	L.
Σ 1424 ... (γ Leonis)	10 14	69 39	113·9	3·79	3	2·0 3·5	·221	W.B.
			113·5	3·62	1	...	·236	H.F.
Σ 1426 ...	10 15	83 4	283·2	0·80	3	7·8 8·3	·243	W.B.
OΣ 216 ...	10 17	74 9	118·4	1·15	2	7·0 10·5	·247	L.
			110·5	1·33	1	...	·304	W.B.
Σ 1429 ...	10 20	64 53	259·8	0·82	3	8·3 8·3	·204	W.B.
OΣ 217 ...	10 21	72 16	150·0	0·94	1	7·3 7·8	·167	B.
			148·0	0·80	2	...	·223	W.B.
			153·4	0·81	1	...	·208	L.
OΣ 218 ...	10 22	85 56	75·5	1·09	1	7·3 9·2	·285	L.
			73·0	1·27	2	...	·307	W.B.
OΣ 224 ...	10 34	80 38	300·7	0·59	3	7·2 9·2	·270	L.
			291·9	0·51	1	...	·268	W.B.
Σ 1457 ...	10 34	83 45	314·2	1·38	3	7·4 8·4	·246	W.B.
			316·0	1·15	1	...	·290	H.F.
OΣ 225 A.B.	10 35	70 14	353·9	6·44	1	7·5 9·8	·140	W.B.
			354·0	6·29	1	...	·208	L.
Perrotin A.C.	...	...	342·5	0·86	1	7·5 11·2	·208	L.
OΣ 227 ...	10 36	78 44	346·3	0·66	3	7·5 8·5	·266	L.
			346·1	0·48	2	...	·286	W.B.
OΣ 228 ...	10 42	66 54	189·9	0·41	2	7·2 8·1	·286	W.B.
			184·7	0·52	1	...	·285	L.
OΣ 229 ...	10 42	48 21	319·5	0·89	1	6·7 7·1	·386	B.
β 596	10 44	72 21	282·9	1·94	1	6·7 11·7	·208	L.
β 915	10 44	65 11	231·7	1·34	2	9·0 9·0	·332	W.B.
Σ 1504 ...	10 59	85 49	111·1	1·28	1	7·5 7·6	·285	L.

Star's Name.	R.A. 1900. h m	N.P.D. 1900. ° ′	Position Angle. °	Distance. ″	No. of Nights.	Mags.	Epoch 1903.	Obs.
			110·7	1·35	1	...	·309	W.B.
Ho. 48... ...	10 59	66 18	6·5	1·58	1	8·3 11·0	·290	H.F.
			9·9	2·17	1	...	·304	W.B.
Σ 1517 ...	11 8	69 19	271·2	0·42	4	7·3 7·3	·285	W.B.
			268·5	0·33	1	...	·288	B.
Σ 1523 ...	11 13	57 54	142·8	2·47	2	4·0 4·9	·178	W.B.
			142·4	2·15	1	...	·290	H.F.
Σ 1527 ...	11 14	75 11	18·0	3·31	4	6·9 8·1	·254	W.B.
			15·2	3·34	1	...	·290	H.F.
Σ 1534 ...	11 17	71 15	325·6	5·44	2	8·0 11·2	·286	W.B.
Σ 1536 ...	11 19	78 55	53·8	2·24	3	3·9 7·1	·241	W.B.
Lalande 21846	11 24	58 59	8·8	1·00	1	7·0 11·5	·304	L.
			8·8	1·09	1	...	·304	W.B.
OΣ 234 ...	11 25	48 10	142·2	0·34	1	7·0 7·4	·419	B.
Σ 1554 ...	11 31	76 36	257·0	0·91	3	8·9 9·1	·294	W.B.
			260·0	0·84	1	...	·285	L.
Σ 1555 ...	11 31	61 40	357·0	0·35	3	6·4 6·8	·272	W.B.
			357·3	0·31	1	...	·285	L.
			351·2	0·27	1	...	·288	B.
β 603	11 44	75 10	313·4	0·83	3	6·4 10·2	·333	L.
Hu. 570 ...	12 9	68 9	108·4	2·04	1	9·0 13·0	·394	L.
Σ 1621 ...	12 11	83 49	138·0	2·71	1	8·8 10·3	·318	L.
			135·2	2·58	1	...	·359	W.B.
Ho. 52 ...	12 16	71 39	46·0	...	1	5·0 13·0	·318	L.
Σ 1639... ...	12 20	63 52	357·7	0·28	1	6·7 7·9	·288	B.
			357·3	0·35	3	...	·344	L.
			360·9	0·27	1	...	·359	W.B.
Σ 1643 ...	12 22	62 15	37·7	1·95	2	8·4 8·7	·196	W.B.
			39·4	1·94	1	...	·290	H.F.
Σ 1647 ...	12 25	79 44	42·0	1·53	2	7·5 7·8	·271	W.B.
Hu. 571 ...	12 27	69 27	84·4	0·37	1	8·8 8·8	·416	W.B.
β 797	12 29	83 39	169·2	0·61	2	8·5 8·5	·328	L.
			160·2	0·59	1	...	·359	W.B.
Σ 1658 ...	12 30	82 0	358·2	2·35	2	8·0 9·8	·334	W.B.
			361·0	2·33	2	...	;328	L.
Σ 1661 ...	12 30	78 3	239·4	2·48	2	8·5 8·5	·277	W.B.
			240·2	2·05	1	...	·290	H.F.
Σ 1663 ...	12 32	68 15	100·7	0·54	2	7·8 8·7	·277	W.B.

Star's Name.		R.A. 1900. h m	N.P.D. 1900. ° ′	Position Angle. °	Distance. ″	No. of Nights.	Mags.	Epoch 1903.	Obs.
Σ 1687	A.B.	12 48	68 13	84·6	1·18	3	5·0 7·8	·346	W.B.
				87·5	0·97	2	...	·348	L.
				85·3	0·88	1	...	·329	H.F.
	A.C.			125·0	28·99	1	5·0 9·0	·329	H.F.
OΣ 256	...	12 51	90 25	79·1	0·60	1	7·2 7·6	·345	L.
				74·6	0·57	1	...	·386	B.
				80·3	0·69	1	...	·405	W.B.
Σ 1711	...	12 58	75 59	347·4	0·93	2	8·5 9·0	·334	W.B.
				351·5	1·03	1	...	·318	L.
β 929 ...	...	12 59	93 7	211·6	0·68	1	6·2 6·2	·378	L.
				213·5	0·53	1	...	·386	B.
				214·1	0·42	1	...	·416	W.B.
OΣ 260	...	13 3	62 31	120·3	0·57	3	7·9 8·3	·350	W.B.
Hu. 572	...	13 4	68 1	...	0·36	2	8·0 9·0	·409	L.
Σ 1728	...	13 5	71 57	196·2	0·33	3	6·0 6·0	·284	W.B.
				187·8	0·57	1	...	·288	B.
OΣ 261	...	13 7	57 23	347·9	1·62	3	6·9 7·4	·421	W.B.
Hu. 573	...	13 8	66 33	174·6	2·38	2	8·5 13·0	·409	L.
β 800 ...	...	13 12	72 27	112·7	2·54	2	7·1 10·2	·356	L.
				113·2	2·83	1	...	·359	W.B.
Σ 1734	...	13 16	86 32	187·7	1·03	1	7·2 7·9	·329	H.F.
				192·3	1·27	1	...	·359	W.B.
				192·8	1·00	1	...	·424	L.
Σ 1742	...	13 19	88 5	353·4	1·33	1	7·4 7·9	·359	W.B.
				354·6	1·18	1	...	·424	L.
Ho. 260	...	13 19	60 15	319·1	0·66	2	8·3 8·5	·418	W.B.
				328·1	0·75	2	...	·414	L.
				314·4	0·76	1	...	·419	B.
OΣ 266	...	13 24	73 47	341·8	1·63	2	7·3 7·8	·329	W.B.
				343·4	1·54	1	...	·329	H.F.
β 113 ...	...	13 24	78 0	193·8	...	1	8·2 11·0	·345	L.
				208·4	1·84	1	...	·496	W.B.
OΣ 269	...	13 28	54 34	218·9	0·26	2	6·5 7·0	·354	B.
				216·9	0·27	1	...	·476	W.B.
Σ 1757	...	13 29	89 48	81·8	2·42	1	7·8 8·9	·359	W.B.
				80·5	2·14	1	...	·389	H.F.
Σ 1768	...	13 33	53 13	310·2	1·18	1	5·7 7·6	·288	B.
β 612 ...	...	13 35	78 45	246·0	0·26	2	6·3 6·5	·358	L.

Star's Name.		R.A. 1900. h m	N.P.D. 1900. ° ′	Position Angle. °	Distance. ″	No. of Nights.	Mags.	Epoch 1903.	Obs.
				243·8	0·20	2	...	·428	W.B.
				252·5	0·22	1	...	·419	B.
Σ 1777	...	13 38	85 57	230·0	3·37	2	5·8 8·2	·358	L.
Σ 1781	...	13 41	84 23	277·7	0·92	2	7·8 8·2	·358	L.
				283·1	1·09	1	...	·375	W.B.
β 801 ...	...	13 42	78 40	327·0	2·41	1	8·2 10·2	·337	L.
Σ 1785	...	13 45	62 31	290·7	1·43	1	7·2 7·5	·318	L.
				287·8	1·21	1	...	·329	H.F.
				291·5	1·43	2	...	·367	W.B.
β 613 ...	...	13 47	54 50	149·6	0·94	1	9·0 9·0	·476	W.B.
Σ 1788	...	13 50	97 34	81·2	2·32	1	6·7 7·9	·389	H.F.
β 30 ...	...	13 53	70 5	200·1	8·57	3	8·1 10·7	·370	L.
β 1270	...	13 59	81 2		Round		8·2 8·3	·345	L.
				22·6	0·19	1	...	·378	L.
				23·5	0·15	1	...	·416	W.B.
Σ 1808	...	14 6	62 56	74·3	2·80	1	8·0 9·0	·318	L.
				73·6	2·80	1	...	·375	W.B.
Σ 1819	...	14 10	86 24	361·1	1·23	2	7·9 8·0	·401	L.
				360·0	1·21	1	...	·389	H.F.
				359·3	1·51	1	...	·416	W.B.
✶ with Σ 1819		14 11	86 12	198·5	1·53	1	7·5 9·5	·416	W.B.
Σ 1832	A.B.	14 14	85 39	135·4	0·61	2	9·0 9·0	·400	L.
				129·1	0·65	1	...	·416	W.B.
	A.C.			72·3	20·18	2	9·0 10·5	·400	L.
Σ 1835	A.B.	14 18	81 6	188·5	6·70	1	5·8 6·8	·422	L.
β 1111	B.C.			36·9	0·25	1	8·2 8·4	·422	L.
Hu. 574	...	14 31	70 19	100·2	0·24	1	8·5 8·8	·416	W.B.
				111·2	0·24	1	...	·422	L.
Σ 3087	...	14 32	70 10	221·9	2·04	1	9·5 9·5	·403	L.
Σ 1865	...	14 36	75 51	153·5	0·33	2	3·5 3·9	·354	B.
				145·8	0·45	2	...	·370	L.
				139·2	0·25	2	...	·378	W.B.
Hu. 575	...	14 38	70 5	168·0	0·80	2	9·0 9·5	·413	L.
Hu. 576	...	14 41	69 25	190·7	4·80	1	8·2 13·0	·403	L.
Σ 1877	...	14 41	62 30	329·8	2·75	2	3·0 6·3	·378	W.B.
				325·5	2·81	1	...	·403	L.
Σ 1879	...	14 41	79 55	131·7	0·52	1	7·8 8·8	·337	L.
				140·2	0·53	1	...	·419	B.

Star's Name.	R.A. 1900. h m	N.P.D. 1900. ° ′	Position Angle. °	Distance. ″	No. of Nights.	Mags.	Epoch 1903.	Obs.
			132·7	0·42	1	...	·476	W.B.
Star following...	14 41	79 47	115·9	0·18	1	7·5 8·5	·337	L.
Σ 1883 ...	14 44	83 37	240·7	0·41	1	7·0 7·0	·381	W.B.
			243·6	0·48	1	...	·419	B.
Σ 1888 ...	14 47	70 29	186·0	2·39	1	4·7 6·6	·381	W.B.
			186·5	2·51	1	...	·389	H.F.
			190·3	2·81	1	...	·403	L.
OΣ 287 ...	14 48	44 40	322·5	0·71	1	7·5 7·6	·345	L.
β 31 A.B.	14 48	70 51	193·1	1·55	1	8·4 9·7	·403	L.
OΣ 288 ...	14 49	73 53	189·5	1·59	2	6·4 7·1	·378	W.B.
			191·3	1·69	1	...	·403	L.
β 348	14 57	89 45	116·1	0·43	1	6·0 7·5	·419	B.
Σ 1909 ...	15 1	41 57	239·5	4·45	1	5·2 6·1	·345	L.
β 1086... ...	15 2	41 28	251·2	6·25	1	5·5 13·5	·345	L.
Hu. 144 ...	15 6	69 16	241·8	0·71	1	8·8 11·0	·496	W.B.
Σ 1932 ...	15 14	62 48	156·3	0·58	2	5·6 6·1	·399	W.B.
			158·1	0·71	2	...	·408	B.
Hu. 146 ...	15 17	68 35	169·6	0·36	1	8·7 9·0	·471	L.
Σ 1937 ...	15 19	59 21	14·4	0·99	1	5·2 5·7	·381	W.B.
			15·1	0·85	2	...	·408	B.
Σ 1938 ...	15 21	52 18	68·7	1·05	2	6·7 7·3	·408	B.
OΣ 296 ...	15 23	45 39	310·0	1·56	1	7·0 8·6	·345	L.
Aitken 82 ...	15 23	65 44	327·3	0·78	1	8·5 9·3	·471	L.
			325·4	0·90	1	...	·512	W.B.
Hu. 577 ...	15 28	69 55	21·5	0·36	2	8·0 8·0	·464	W.B.
			18·8	0·40	1	...	·470	L.
Σ 1954 ...	15 30	79 7	187·5	3·51	1	3·0 4·0	·381	W.B.
			186·8	3·65	3	...	·451	H.F.
Σ 1957 ...	15 31	76 44	158·1	1·31	1	8·0 9·5	·381	W.B.
			158·4	1·03	1	...	·444	H.F.
OΣ 298 ...	15 33	49 52	185·5	1·17	1	7·0 7·3	·345	L.
			184·0	1·15	2	...	·408	B.
Hu. 580 ...	15 37	70 0	67·2	0·25	2	5·0 5·0	·464	W.B.
			70·2	0·17	1	...	·471	L.
β 619	15 39	76 1	6·7	0·45	2	6·5 7·0	·447	W.B.
			4·1	0·52	1	...	·419	B.
			3·3	0·69	1	...	·444	H.F.
			2·3	0·55	1	...	·471	L.

Star's Name.	R.A. 1900.	N.P.D. 1900.	Position Angle.	Distance.	No. of Nights.	Mags.	Epoch 1903.	Obs.
	h m	° ′	°	″				
Σ 1967 ...	15 39	62 23	118·7	0·58	1	4·0 7·0	·381	W.B.
			116·2	0·75	1	...	·419	B.
β 621	15 47	45 8	51·7	0·42	1	7·5 8·0	·345	L.
β 810	15 48	47 14	91·4	1·09	1	8·7 11·5	·345	L.
OΣ 303 ...	15 56	76 27	144·2	0·80	2	7·4 7·9	·399	W.B.
β 355	16 5	44 21	274·4	0·44	1	7·8 8·9	·345	L.
Σ 2021 ...	16 8	76 12	334·0	3·67	1	6·7 6·9	·416	W.B.
			334·7	4·11	2	...	·482	H.F.
Hu. 481 ...	16 17	66 16	224·6	0·42	1	7·3 9·2	·471	L.
			229·2	0·40	1	...	·512	W.B.
Hu. 482 ...	16 18	67 28	148·4	1·58	1	9·0 13·8	·496	W.B.
β 951	16 20	56 23	61·5	0·98	1	8·2 8·7	·345	L.
OΣ 312 ...	16 22	28 16	142·1	5·30	1	2·1 8·1	·383	L.
Hu. 483 A.B.	16 23	68 53	296·2	1·84	1	8·2 13·0	·496	W.B.
OΣ 311 A.C.	16 23	68 53	203·5	6·56	1	8·2 10·3	·496	W.B.
			200·9	6·44	1	...	·518	L.
Σ 2054 ...	16 24	28 4	9·7	0·96	1	5·7 6·9	·383	L.
Σ 2052 ...	16 25	71 23	94·1	1·42	1	7·5 7·5	·490	L.
			90·7	1·59	1	...	·559	W.B.
Σ 2055 ...	16 26	87 48	61·6	1·15	1	4·0 6·1	·490	L.
			58·5	1·46	1	...	·559	W.B.
Hu. 486 ...	16 35	67 4	138·3	1·19	2	9·0 10·0	·504	W.B.
Σ 2084 ...	16 36	58 13	200·6	1·14	1	3·0 6·5	·383	L.
(ζ Herculis)			201·0	0·95	1	...	·419	B.
			197·5	0·78	1	...	·676	W.B
De. 15	16 41	46 20	310·5	0·42	1	8·2 8·6	·383	L.
OΣ 315 ...	16 46	88 37	157·6	0·86	1	6·2 8·1	·444	H.F.
			155·0	0·75	1	...	·559	W.B.
β 627	16 46	43 51	321·5	1·48	1	5·0 9·5	·383	L.
Σ 3107 ...	16 54	85 53	95·5	1·39	2	8·5 8·5	·482	H.F.
Σ 2118 ...	16 56	24 49	204 ±	...	1	6·4 6·9	·383	L.*
			89·1	0·13	1	...	·419	B.
Σ 2114 ...	16 57	81 24	161·8	1·23	1	6·2 7·4	·444	L.
Σ 2130 A.B.	17 3	35 24	135·9	2·23	1	5·0 5·1	·383	L.
β 1088 B.C.			189·0	12·37	1	5·1 13·5	·383	L.
Σ 2140 ...	17 10	75 30	110·5	4·68	2	3·0 6·1	·482	H.F.

* Suspected.

Star's Name.	R.A. 1900. h m	N.P.D. 1900. ° ′	Position Angle. °	Distance. ″	No. of Nights.	Mags.	Epoch 1903.	Obs.
Σ 2145 A.B.	17 12	63 18	30·0	0·45	2	8·3 8·9	·566	L.
$\frac{A.B.}{2}$ & C.			181·3	13·74	2	9·3	·566	L.
OΣ 327 ...	17 12	33 45	321·0	0·32	1	7·6 7·9	·383	L.
Σ 2147 ...	17 13	61 0	93·7	6·35	1	7·1 11·0	·613	L.
Hu. 172 ...	17 13	78 40	344·4	0·70	1	9·2 11·7	·518	L.
Σ 2157 ...	17 18	73 27	207·7	3·29	1	8·3 9·7	·613	L.
Σ 2160 ...	17 21	74 18	61·6	3·97	1	5·5 10·0	·613	L.
β 1250 ...	17 21	59 9	73·5	2·09	1	9·4 9·5	·501	W.B.
Σ 2165 ...	17 22	60 28	53·7	8·03	1	7·0 8·5	·613	L.
OΣ 331 ...	17 27	86 6	343·2	0·99	1	7·5 9·0	·520	H.F.
Σ 2182 ...	17 28	66 2	358·0	5·35	1	8·2 9·2	·613	L.
Σ 2190 ...	17 32	68 57	25·5	10·11	1	6·0 9·5	·613	L.
Σ 2194 ...	17 37	65 28	174·8	14·26	1	6·2 8·3	·682	L.
Σ 2205 ...	17 40	72 14	303·7	2·01	1	8·3 8·7	·518	L.
			308·2	2·16	2	...	·540	H.F.
Σ 2215 ...	17 41	72 15	288·5	0·55	1	5·9 7·9	·518	L.
Ho. 70	17 42	59 25	289·0	0·41	1	8·0 8·0	·419	B.
			281·5	0·36	1	...	·471	L.
			282·5	0·35	1	...	·501	W.B.
A.C. 8	17 49	60 18	56·7	0·26	1	8·0 9·8	·471	L.
			58·6	0·36	1	...	·501	W.B.
Aitken 234 ...	17 49	64 23	24·9	0·42	1	8·8 9·1	·471	L.
A.C. 9	17 50	60 10	63·3	1·54	1	8·3 8·7	·671	L.
Σ 2272 ...	18 0	87 28	198·4	1·93	1	4·1 6·1	·444	H.F.
			198·1	1·73	13	...	·573	W.B.
OΣ 341 ...	18 2	68 34	87·6	0·37	1	6·4 7·7	·419	B.
			93·4	0·32	1	...	·594	W.B.
A.C. 15 ...	18 3	59 27	314·8	1·33	1	5·4 9·0	·501	W.B.
Σ 2283 ...	18 4	83 52	80·8	0·81	1	7·2 7·7	·518	L.
			85·7	1·12	4	...	·550	W.B.
Σ 2281 ...	18 5	86 2	234·6	0·20	3	6·7 7·2	·624	W.B.
Σ 2289 ...	18 6	73 32	226·7	1·07	1	6·0 7·1	·518	L.
			228·4	1·21	3	...	·522	W.B.
			232·8	1·16	1	...	·520	H.F.
Σ 2309 ...	18 16	64 31	351·0	4·25	1	8·5 9·0	·630	W.B.
Σ 2310 ...	18 16	67 13	237·3	5·22	1	7·0 10·3	·630	W.B.
Σ 2312 ...	18 17	61 43	333·5	1·60	1	8·5 9·5	·630	W.B.
			338·9	1·34	1	...	·671	L.

Star's Name.		R.A. 1900. h m	N.P.D. 1900. ° ′	Position Angle. °	Distance. ″	No. of Nights.	Mags.		Epoch 1903.	Obs.
β 641 ...	...	18 18	68 32	345·6	0·90	2	7·5	9·0	·623	L.
Σ 2314	...	18 19	66 36	322·8	2·22	1	8·4	9·6	·671	L.
Σ 2315	...	18 21	62 39	193·0	0·23	1	7·0	8·0	·419	B.
				197·6	0·18	1	...		·575	L.
Σ 2319	...	18 23	70 46	192·6	5·53	1	7·2	7·6	·671	L.
Σ 2336	...	18 28	76 15	8·3	6·25	1	8·7	9·8	·671	L.
Σ 2339	...	18 29	72 21	274·7	2·11	1	7·2	8·0	·671	L.
Σ 2345	...	18 31	69 0	204·9	8·65	1	8·4	10·1	·635	W.B.
OΣ 357	...	18 31	78 21	240·8	0·43	3	7·5	7·6	·567	W.B.
OΣ 358	...	18 31	73 6	12·0	1·82	3	6·8	7·2	·503	W.B.
Σ 2356	...	18 34	61 23	55·7	1·15	1	8·0	9·0	·635	W.B.
Σ 2358	...	18 35	59 22	221·3	2·82	1	8·8	9·0	·635	W.B.
Σ 2360	...	18 35	69 9	360·8	2·45	1	7·5	8·7	·635	W.B.
Σ 2364	...	18 36	65 23	178·2	7·50	1	8·0	10·2	·635	W.B.
Σ 2367	A.B.	18 37	59 48	69·3	0·13	1	7·0	7·5	·419	B.
				78·5	...	1	...		·575	L.
				70·2	0·19	4	...		·655	W.B.
	A.C.	18 37	59 48	193·3	14·22	1	7·0	8·2	·635	W.B.
Σ 2371	...	18 38	62 28	235·9	9·53	1	8·5	8·5	·635	W.B.
Σ 2369	...	18 39	87 29	94·6	1·04	1	7·5	8·0	·575	L.
Σ 2375	...	18 41	84 36	113·0	2·12	2	6·2	6·6	·589	H.F.
				113·6	2·15	1	...		·594	W.B.
Σ 2400	A.D.	18 44	73 52	184·8	2·27	1	8·1	10·6	·575	L.
	A.C.	...	...	186·3	3·68	1	8·1	11·0	·575	L.
	B.C.	...	...	183·2	0·85	1	10·6	11·0	·575	L.
Σ 2402	...	18 45	79 25	207·3	0·89	1	8·0	8·4	·594	W.B.
Σ 2422	...	18 53	64 2	92·4	0·81	3	7·6	7·7	·524	W.B.
				95·9	0·88	1	...		·657	H.F.
Σ 2437	...	18 58	70 58	59·7	0·76	3	7·8	8·0	·549	W.B.
Σ 2454	...	19 2	59 43	239·3	0·84	1	8·0	9·2	·676	W.B.
Σ 2455	...	19 3	67 59	77·3	3·42	3	7·2	8·3	·579	W.B.
				77·0	3·45	3	...		·638	H.F.
Σ 2488	...	19 11	70 9	327·6	1·71	1	8·5	9·7	·559	W.B.
OΣ 368	...	19 12	74 1	217·3	0·82	3	7·3	8·5	·581	W.B.
OΣ 371	...	19 12	62 42	153·6	0·74	4	6·8	6·9	·561	W.B.
Σ 2525	...	19 23	62 52	308·8	0·61	2	7·4	7·6	·659	W.B.
OΣ 375	...	19 30	72 7	145·3	0 64	1	7·2	8·4	·676	W.B.
Σ 2587	...	19 46	86 10	101·9	3·84	3	6·5	9·2	·699	H.F.

Star's Name.	R.A. 1900. h m	N.P.D. 1900. ° ′	Position Angle. °	Distance. ″	No. of Nights.	Mags.	Epoch 1903.	Obs.
			103·2	4·42	1	...	·720	L.
Ho. 580 ...	19 48	67 49	280·3	0·64	1	8·0 8·1	·802	B.
Σ 2600 ...	19 51	67 46	58·7	2·94	1	8·3 9·7	·734	H.F.
			58·2	2·91	1	...	·753	W.B.
A.C. 16 ...	19 54	63 2	57·7	0·40	1	7·8 8·2	·739	L.
			62·4	0·30	1	...	·753	W.B.
Σ 2616 ...	19 58	75 42	265·6	3·61	2	6·8 9·7	·605	W.B.
			265·1	3·06	1	...	·734	H.F.
OΣ 395 ...	19 58	65 21	102·3	0·46	1	5·5 6·0	·753	W.B.
			100·8	0·58	2	...	·791	B.
Σ 2620 ...	19 59	78 30	288·2	1·99	2	8·2 9·3	·605	W.B.
			290·9	1·62	1	...	·734	H.F.
Σ 2624 ...	20 0	54 15	176·5	1·88	1	7·2 7·8	·616	W.B.
Aitken 382 ...	20 5	47 54	89·9	1·21	1	7·0 10·0	·498	L.
Hu. 585 ...	20 13	39 10	52·4	4·22	1	9·0 10·0	·498	L.
*	20 25	41 58	349·9	4·92	1	8·5 9·5	·498	L.
Σ 2695 ...	20 28	64 32	80·6	1·21	4	6·2 8·⊖	·725	W.B.
			80·0	1·12	2	...	·671	L.
			83·2	1·11	1	...	·734	H.F.
Σ 2698 ...	20 30	62 13	283·7	3·89	1	8·1 9·0	·613	L.
* near Σ 2698 A.B.	20 31	62 5	111·9	3·97	1	9·0 10·0	·613	L.
* near Σ 2698 A.C.			198·3	9·53	1	9·0 10·5	·613	L.
β 151 A.B.	20 33	75 45	32·0	0·36	3	4·5 6·0	·820	B.
			30·2	0·28	2	...	·822	W.B.
A.C.			120·2	25·30	1	4·5 12·7	·739	L.
Hu. 588 ...	20 35	39 58	244·3	1·86	1	9·0 11·0	·498	L.
* near Hu. 588			87·1	4·32	1	8·5 10·0	·498	L.
Σ 2737 A.B.	20 54	86 5	278·8	0·66	2	5·7 8·6	·791	B
			285·9	0·53	1	...	·841	W.B.
A.C.			74·3	11·24	1	5·7 7·4	·720	L.
			73·8	10·43	1	...	·841	W.B.
Σ 2739 ...	20 55	70 20	251·2	3·04	1	8·3 8·8	·879	B.
Σ 2742 ...	20 57	83 14	222·0	2·97	1	7·1 7·1	·879	B.
Σ 2744 ...	20 58	88 52	158·0	1·40	1	6·3 7·0	·841	W.B.
			164·0	1·24	1	...	·879	B.
Σ 2750 ...	21 0	77 41	280·4	15·68	1	7·8 9·3	·879	B.
Σ 2754 ...	21 1	77 14	300·3	32·72	1	8·0 8·7	·879	B.

Star's Name.	R.A. 1900.	N.P.D. 1900.	Position Angle.	Distance.	No. of Nights.	Mags.	Epoch 1903.	Obs.
	h m	° ′	°	″				
Σ 2767 ...	21 6	70 27	29·8	2·30	1	7·8 8·2	·701	L.
Σ 2769 ...	21 6	67 58	209·2	18·16	1	6·5 7·5	·701	L.
Σ 2765 ...	21 6	80 51	265·9	2·73	2	7·8 8·0	·772	L.
			266·3	2·80	1	...	·879	B.
OΣ 535 A.B. (δ Equulei)	21 10	80 24	23·8	0·34	6	4·1 4·1	·775	L.
			25·1	0·29	6	...	·812	W.B.
			26·4	0·30	4	...	·838	B.
			27·4	0·35	2	...	·863	H.F.
Σ 2785 ...	21 14	50 40	242·7	2·97	1	8·1 10·0	·882	L.
Σ 2797 ...	21 22	76 45	211·7	2·94	1	6·7 8·2	·701	L.
Σ 2799 ...	21 24	79 21	295·5	1·42	2	7·0 7·0	·758	W.B.
			297·5	1·26	2	...	·852	H.F.
Σ 2804 ...	21 28	69 44	333·8	2·84	4	7·3 8·0	·681	W.B.
			336·9	2·51	1	...	·827	H.F.
Σ 2808 ...	21 31	59 23	173·4	...	1	...	·682	L.
Ho. 163 ...	21 32	58 50	42·6	6·64	1	8·0 13·0	·682	L.
β 167 ...	21 32	60 24	87·3	1·94	1	7·0 12·0	·682	L.
β 1212 ...	21 34	90 30	271·7	0·47	2	6·5 6·9	·823	B.
Σ 2814 ...	21 35	54 5	162·6	7·82	1	8·3 9·8	·663	L.
			158·1	7·96	1	...	·665	W.B.
Ho. 166 ...	21 40	62 38	85·5	0·30	2	7·5 7·5	·773	L.
			83·5	0·34	2	...	·759	W.B.
Σ 2822 ...	21 40	61 42	123·0	2·37	3	4·0 5·0	·753	W.B.
			125·8	2·38	1	...	·663	L.
			125·5	2·29	1	...	·827	H.F.
β 989 A.B. (κ Pegasi)	21 40	64 49	137·6	0·18	5	3·9 4·4	·740	L.
			131·4	0·25	5	...	·833	W.B.
			125·1	0·18	3	...	·850	B.
Σ 2824 A.C.			297·4	12·85	4	3·9 10·8	·701	L.
Σ 2825 ...	21 42	89 36	115·1	0·97	1	8·0 8·2	·808	L.
Σ 2829 ...	21 45	59 43	195·2	17·41	1	8·2 9·2	·882	L.
Σ 2834 ...	21 47	71 10	300·6	3·77	1	7·3 10·6	·882	L.
Ho. 171 ...	21 48	62 41	354·5	0·76	2	8·2 8·2	·773	L.
Σ 2846 ...	21 51	44 42	274·3	3·24	1	8·5 10·3	·882	L.
β 75 ...	21 51	79 35	43·7	0·90	1	8·1 8·3	·876	H.F.
Σ 2866 ...	22 5	49 50	55·0	8·91	1	8·3 11·3	·665	W.B.
Σ 2878 ...	22 10	82 31	125·1	1·31	1	6·5 9·0	·808	L.
Σ 2882 ...	22 10	52 45	327·4	3·17	1	9·2 9·2	·665	W.B.

Star's Name.		R.A. 1900. h m	N.P.D. 1900. ° ′	Position Angle. °	Distance. ″	No. of Nights.	Mags.		Epoch 1903.	Obs.
Σ 2881 ...		22 10	60 55	98·1	1·67	2	7·7	8·2	·711	W.B.
				100·0	1·34	2	...		·838	H.F.
Ho. 180 ...		22 12	46 38	222·8	0·58	1	7·2	7·2	·991	L.
β 172		22 19	95 21	6·6	0·59	2	5·6	6·0	·885	B.
Σ 2900 ...		22 19	69 39	178·5	1·36	2	6·0	9·2	·831	L.
Σ 2902 ...		22 19	45 9	91·0	6·50	1	7·1	8·0	·657	H.F.
Σ 2906 ...		22 22	53 4	1·5	4·15	1	7·0	10·6	·665	W.B.
Σ 2908 ...		22 23	73 15	116·0	8·85	1	7·0	8·7	·841	W.B.
Σ 2910 ...		22 23	66 59	339·1	5·49	1	8·3	8·8	·841	W.B.
β 701		22 23	78 16	262·3	1·07	1	7·5	10·2	·663	L.
Σ 2912 ...		22 25	86 5	285·6	0·33	1	5·8	7·2	·676	W.B.
Σ 2916	A.B.	22 27	49 18	335·7	44·88	1	7·3	8·8	·657	H.F.
	B.C.	...	...	32·7	3·97	1	8·8	10·2	·665	W.B.
Ho. 296 ...		22 36	75 59	79·2	0·34	2	5·5	5·5	·813	B.
				72·9	0·36	3	...		·853	W.B.
Σ 2934 ...		22 37	69 5	140·5	1·01	1	8·2	9·2	·676	W.B.
				142·8	0·86	2	...		·831	L.
Σ 2946 ...		22 45	50 1	254·9	5·25	1	8·0	8·0	·665	W.B.
Σ 2945 ...		22 45	59 13	293·7	4·14	1	8·5	8·5	·665	W.B.
β 382		22 49	45 47	237·4	0·89	1	6·9	8·5	·991	L.
β 383	A.B.	22 51	81 5	114·6	2·96	1	8·0	12·7	·950	L.
	A.C.	...	...	243·5	16·47	1	8·0	12·8	·950	L.
OΣ 536 ...		22 54	81 10	349·8	0·28	1	7·0	7·5	·923	B.
				342·2	0·32	1	...		·950	L.
OΣ 483 ...		22 54	78 48	228·9	0·91	4	6·2	7·7	·864	W.B.
				224·3	0·98	1	...		·849	H.F.
Σ 2968 ...		22 56	59 27	89·0	3·25	1	7·0	9·5	·665	W.B.
Σ 2974 ...		23 0	57 10	161·5	2·70	1	8·0	8·0	·665	W.B.
Σ 2978 ...		23 3	57 43	145·2	8·33	2	6·8	8·0	·780	W.B.
Σ 2990 ...		23 8	68 28	63·3	1·93	1	8·5	8·5	·895	W.B.
β 79		23 12	92 4	81·1	0·91	2	8·0	8·6	·885	B.
β 80		23 14	85 8	63·5	0·23	2	8·2	9·1	·885	B.
Σ 3012 ...		23 23	73 55	188·5	2·80	1	8·7	8·8	·895	W.B.
Σ 3013 ...		23 23	73 55	275·7	3·31	1	7·8	9·3	·895	W.B.
β 1266	A.B.	23 25	59 44	30·9	...	1	7·4	7·4	·950	L.
				22·6	0·13	1	...		·953	W.B.
Σ 3018	A.C.	...	...	203·0	19·30	1	7·4	9·0	·950	L.
β 720		23 29	59 14	348·1	0·35	2	6·0	6·5	·852	B.
				353·4	0·36	2	...		·935	W.B.
				344·3	0·38	1	...		·950	L.

Star's Name.		R.A. 1900. h m	N.P.D. 1900. ° ′	Position Angle. °	Distance. ″	No. of Nights.	Mags.	Epoch 1903.	Obs.
β 858	A.B.	23 36	58 0	252·3	0·92	1	7·7 8·2	·950	L.
				256·2	0·57	1	...	·953	W.B.
β 389	A.C.	...	...	255·4	23·37	1	7·7 10·8	·950	L.
A.G.C. 14	...	23 39	61 12	193·8	1·81	2	5·0 9·5	·926	W.B.
				195·4	1·67	1	...	·950	L.
Barnard	...	23 42	85 19	171·3	0·54	1	8·6 8·6	·846	B.
Σ 3050	...	23 54	56 0	215·5	2·46	3	6·0 6·0	·893	L.
				216·4	2·30	1	...	·827	H.F.
				215·1	2·25	3	...	·932	W.B.
Σ 3056	A.B.	23 59	56 17	156·3	0·50	1	7·4 7·4	·824	L.
				152·6	0·66	3	...	·922	W.B.
	A.C.	...	...	357·4	22·71	1	7·4 9·0	·824	L.
	$\frac{A.B.}{2}$ C.	...	...	359·7	22·50	1	7·4 9·0	·739	L.

Report on the Work of the Argentine National Observatory, 1904.
By the Director, John M. Thome.

(*Communicated by the Astronomer Royal.*)

In the twelve months ending with March, after deducting the losses occasioned by the Moon and unfavourable weather, there remained 93 nights, long and short, available for photographic and Durchmusterung work; for general meridian work, in which a moonless and cloudless sky is not essential, 180 nights were recorded.

During this limited period every effort was made to secure the maximum of results, and we obtained 298 plates, 108 of which were for the atlas and contained three exposures of thirty minutes', or longer, duration, according to the steadiness of the images and the transparency of the atmosphere at the time. Long-exposure plates are taken whenever the time disposable and the atmospheric conditions warrant the risk.

Both the Repsold-Gill and the Gautier machines have been in constant use, and we have measured the coordinates of 34,100 stars, in both positions of the plates, with an approximation to thousandths of a millimetre; and also the diameters of the images, according to Gill's method. Over two-thirds of these measures correspond to the Gill machine. Two observers take part in the measurement of every plate, reading alternate 5′ zones; each observer, again, measuring the same 5′ zone when the plate is reversed. The images of the reference stars, which have been previously marked upon the plate, are read by both observers. Nine hours, including an interval of two hours at noon, represent a day's work.

Observing alone, as heretofore, I have added 65,000 positions to those already recorded in the Dm. Zone 52°–62°, and with these the observing is essentially finished. This brings the total number of determinations of position closely up to 400,000, representing about 138,000 stars. The revision of these results, especially with regard to the magnitudes, is finished for something more than one-third of the region; and, if the weather will permit, I shall finish everything by the end of the year. My original intention was to give estimates for all the Cape stars not observed here, but I soon found that that would not be possible in the Milky Way, where the lowest limit extends beyond $11\frac{1}{2}$, and the objects are too numerous and would require too much time to classify. As already stated, the Cape invariably leads in the galactic region, but in the non-galactic Cordoba has many more stars. The Cordoba scale is essentially the same in both regions, but the photographic exposures would seem either to have been somewhat longer, or the plates were more sensitive, in the crowded regions, since the corresponding lower limits differ by more than one and a half magnitude.

There is also in preparation an atlas for this belt. Something more than half the work has already been accomplished, and I hope that by the end of March the entire 280,000 stars will have been plotted. In the crowded regions of the Milky Way additional maps, drawn to twice the original scale, have been added, and the entire work has been under my constant supervision, which was not the case with the first atlas.

The registering micrometer, with clock-work attachment, arrived at the beginning of January, and after a few modifications could be made in the arrangement for fastening it to the tube, practice work was at once begun. The method has two decided advantages over the old one. The relative speed that must be given to the movable thread to make it coincide, or very nearly, with that of a star at any declination can be indicated in advance upon a scale, and a careful observer will soon be able to control the rate, so as to assure exact bisections of the images, which will then give accordant determinations of the clock error from both time and circumpolar stars. The second advantage lies in the fact that the observer is not conscious of the instants of transit, as in the old method, and is, therefore, not subjected to the recurrent strains, or shocks, at those critical moments; the strain is a uniform one throughout, and the attention of the observer is exclusively given to the bisection.

The value of a revolution of the screw is $4^s{\cdot}655$ in equatorial interval, and there are ten contacts recorded, automatically, in a revolution. Consequently it was necessary to use the two-pen system, in order to guard against confusion and to assure accurate readings of the contacts. Our Hipp chronograph carries two pens, but both its record and that of the Bond chronograph, which we have used ever since commencing Gould's zones, have a lineal length of only eight millimetres between second breaks,

which is not sufficient to ensure readings accurate to the hundredth-part of an interval. Fortunately our clockmaker was able to give a speed to the barrel corresponding to twice that interval, or sixteen millimetres to a second, which is sufficient for all purposes. The Hipp chronograph enables one also to detect any error in the alignment of the pens at any moment by simply moving the carriage laterally and noting the trace or traces.

The illumination of the reticule and microscopes of our meridian circle had always been unsatisfactory; at times the transits being noted with difficulty, until we tried a lamp in which the illuminant is vapour of carburetted alcohol impinging on a gauze wick. This was still not entirely satisfactory for the microscopes, the illuminating mirrors for which had become tarnished, and I then decided to fix a small electric lamp, enclosed in a wooden box with an opening in the side directly facing the reflecting surface of each microscope, and this arrangement has given perfect satisfaction. The lamps are operated by the ordinary key, and are employed only during the few moments required in reading the microscopes. With the enlargement, besides, of the meridian room to four times its original length, of which I gave an account in my former report, provided throughout with sliding shutters and doors which can be opened in sections, or altogether, so as to maintain a proper ventilation and a uniform temperature with the outside air, everything has been done that I can think of to increase the accuracy of the results from this notable instrument, which has probably furnished a larger number of stellar positions (over 400,000 in both coordinates) than any other meridian circle in the world. It has suffered no injury during the thirty-two years that I have known it, except the unavoidable friction on the pivots, which has been guaranteed against by counterpoises as much as possible.

Under the original conditions indicated (of instrument and observing-room) the reobservations of the Dm. stars to 9·3 between 22° and 37°, giving nearly 156,000 positions, have been made, excepting that the heat escaping from the tube of the illuminating lamp was carried out of the room by a pipe with a funnel-shaped mouth, which is fixed directly above the lamp in a niche in the wall, and that the meridian shutters in the north and south walls of the room remained open, full or half-length, as the conditions—wind, dust, &c.—during the course of work permitted.

The method followed in these observations is not as complete and accurate as when all four microscopes are used, and to obtain the best results two observers and two microscope-readers, carefully trained and in hearty sympathy with the work, should perform the whole, alternating nightly. That condition I have found it impossible to maintain here.

This great mass of results obtained from the collaboration of many observers requires great care and much time in the treatment, and so long as there remain any errors to be detected

in the records, or the methods employed may be varied to bring about more accordant and homogeneous results, I see no good reason for advancing their publication. The delay in the various processes is occasioned entirely by the difficulty in finding reliable and competent assistance, and my best men are getting old and are falling off. In spite of every precaution which I had taken to secure accuracy in the constants of reduction the determination of many of them turned out to have been carelessly made, which has compelled a repetition of large pieces of the original computations. The final results of observation (after a careful revision of all the processes) of Auwers's fundamental stars have been sent to him, and in quality are equal to anything published here.

In the Report of the Committee on Southern and Solar Observatories, published by the Carnegie Institution, the statement is made that "a strong National Observatory was organised at Cordoba in 1870, under the direction of Dr. B. A. Gould, which ranked among the leading observatories of the world for many years; but the financial disasters of Argentina have had a depressing effect upon that observatory," &c.

As this statement might seem to the uninformed to imply that the present administration has not been as alert as the first, I ask leave to state here the exact facts in our case.

When Dr. Gould resigned, in 1885, the observatory staff was, in effect, disbanded. I, who had accompanied Dr. Gould from the beginning, was promoted to the Directorship, Mr. Davis was appointed Director of the Meteorological Office, and Mr. Stevens, the third member of the last staff, was dead. There remained Mr. Tucker, lately arrived, whose time was soon entirely taken up with the affairs of the Dm.; and Mr. Jefferson, a young man who had not had any previous practice in meridian work and could not begin it here until after I assumed the direction, was the only one available for that work. The meridian observations began, therefore, under these depressing circumstances, which were aggravated by the rapid approach of the disastrous financial crisis, complicated by the boundary question with Chile, from which we are just emerging; and my efforts to increase the staff by engaging men abroad upon a paper basis were without permanent result.

Upon the retirement of Dr. Gould I offered to revise and publish the annual catalogues, commencing with 1874—eight volumes—and also to revise and compare the proofs of the *General Catalogue* with the originals here. This revision was a most thorough and exhaustive one, and the entire force at my disposal was engaged upon it, with the result that we were able to detect several hundred important errors. The annual catalogues were subjected to a similar scrutiny, and no one else could have, nor probably would have, given them this preferential attention. The last volume, No. xv., was published in 1896, and I extract the following passage from the preface, since it

seems necessary to again call attention to it. See also the preface to vol. xiii. p. 13.

"The publication of these annual catalogues, beginning with 1874, has occupied a large part of our time, and has been a very considerable drain upon our steadily diminishing resources. The effort to maintain the activity and honourable record of the institution under the smaller salaries consequent upon a vanishing budget, which cut off from me all hope of obtaining trustworthy help abroad, has been attended with painful experiences, and I have repeatedly been compelled to stop and train new men, and to repeat operations performed by inefficient, negligent, or designing assistants."

And in the introduction to vol. xviii., published in 1900 March 15, I stated that the observations were completed in 1897, but that the Minister of Public Instruction had considered it necessary, in view of the possibility of a war over the Chilian boundary question, to reduce by two-thirds our already minimum appropriation, and that I had only recently succeeded in having the original appropriation restored.

At the time I was appointed to the directorship the value of the gold dollar was 0·82 cent; from then until 1889 the average value was 0·72; from 1889 to 1897 this was 0·34, being at one time as low as 0·22, and averaging only 0·28 from 1891 to 1895. For the whole period from March 1885 to the end of 1900 the average was 0·41, and it is now fixed at 0·44. We have always been paid in paper!

During the first administration there were published the *Uranometria Argentina*, the *Zone Catalogue*, and the *General Catalogue*. The observing books show that there was an average of 270 working nights per year during this period, and that the number of stellar positions determined was as follows:—

For the *Zone Catalogue* . .	105,252	observations.
„ „ *General Catalogue* .	160,506	„
Total . . .	265,758	„

During the present administration there have been produced four volumes of the *Cordoba Dm.*, with a total of 630,000 stellar positions and magnitudes, resulting from over 1,800,000 observations; and there are ready eighteen charts, containing 550,000 stars.

The number of meridian observations obtained is 198,000, all reduced to the mean epoch of 1900.

Now, the actual time and labour required to reduce, collect, and prepare for the press that number of Dm. observations, and to publish the results as given in our volumes, is very considerable—apart from the preparation of the atlas—being, in fact, considerably greater than that given to the Uranometry.

There remains, then, the discrepancy between the previous 265,000 observations and the present 198,000; in explanation of

which there is this to be said: Owing to the time and expense which the revision and publication of the above eight catalogues entailed, besides the difficulty in obtaining assistance, it was not possible to begin systematic meridian work before the end of 1890, and in consequence only 28,300 observations were made previously to 1891.

From 1891 to 1901 was the period of our greatest activity, and with an average of no more than 189 working nights per year we obtained 144,151 complete observations. The average in the former period was 270, or a difference of 91 working nights per year, which, in ten years of such weather as we had and are having, amounts to 4·8 years! In other words, ten years in the former period were equal to 14·8 years at the present time. Therefore the number of observations which we assuredly would have obtained under those conditions, and which were lost solely on account of the altered climatic conditions, is 69,192.

We have then—

Number of observations previous to 1891 .	28,300
„ „ from 1891 to 1901 .	144,150
Loss by change of climate	69,192
Number of observations since 1901 . .	30,940
Total	272,582
Gould's total	265,758

all reduced to similar conditions for the purposes of exact comparison.

From the above it should be apparent that not only the "financial disaster," which made it impossible for me ever to maintain as efficient a corps of assistants as Dr. Gould always had, but also climatic "disasters," and the additional burden of publishing the greater part of the volumes produced in that period, have been prime factors in bringing about the state of affairs which the Carnegie Commission presents to your consideration.

Surely no astronomer could wish those gentlemen anything but the most complete and abundant success in the projected enterprise, and the great need of the observatory is sufficiently indicated by the disparity in the numbers of such institutions in the two hemispheres.

Cordoba: 1904 *May* 25.

Erratum (*in some copies*)

Page 618, line 17 from bottom:

The semicolon should be placed after the word "another," and not after "lenses"; the line should therefore *read*

"less independent of one another; in these Cooke lenses, provided"